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ORIGINAL LECTURES.

PROLAPSE OF THE VAGINAL WALLS, DUE TO LACERATION OF THE CERVIX AND INJURY TO THE VAGINAL OUTLET.

A Clinical Lecture delivered at the Jefferson Medical College Hospital, January 12, 1886.

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PROFESSOR THEOPHILUS PARVIN introduced the lecturer with the following words:

GENTLEMEN, the names of the two Americans who are most identified with the advance of knowledge in the diseases of women, are names of gentlemen who have graduated at the Jefferson Medical College. Further, not only in this country are these names most identified with this branch of medicine, but throughout the whole world they stand preëminent among those who have advanced our knowledge of the diseases of women.

One of these gentlemen, Dr. Marion Sims, passed away two years ago; the other is with us to-day to teach you and to teach me; and if there be a man in all the profession whom I especially call master, it is he whose lessons I have tried to teach, and whose lessons I shall continue to teach. To-day you have the opportunity of seeing and hearing the master himself, Dr. Thomas Addis Emmet.

DR. EMMET said:

GENTLEMEN, I am sure that I cannot find words to reply,—to say what I would like to. I feel it a great compliment to be here to-day, which is something that I could never have dreamt of in my student days. But I am almost like Rip Van Winkle, for it is thirty-six years since I left this college, and I am sorry to say that this is the first time that I have been in it since I was graduated. The professors, and I believe all that were then connected with the school, are dead. I greatly appreciate the compliment of being asked to speak to you, but as we have a great deal to do, I shall not waste any more time but proceed to business.

CASE I.—I have just examined the case which is being put under ether. It is a case of procidentia, occurring in an elderly woman, that is prolapse, entire, of both the vagina and uterus. There are several causes for this condition, but it is generally due to a laceration of the cervix. I believe that, as a rule, procidentia occurring in women who have borne children, is due to injury of the cervix. We have, however, exceptions to the rule in women who have not borne children. The condition may be then brought about by lifting and straining where there is a small fibroid favorably situated, or with the bladder distended.

Various methods for holding up the uterus have been employed. If a proper pessary could be devised, it would not be necessary to resort to any surgical measures; but unfortunately mechanical means do not answer. The object of any procedure to cure procidentia, is to place the uterus in such a position that the circulation can be reëstablished, so that the involution, which has been arrested, may go on. You may have a case of marked procidentia in a woman whose last child was born twenty years previous, in whom there is apparently no laceration, yet the line will be there, and if the dense tissue is removed from the cervix and the parts brought together, in about forty days, as the Bible points out, it will be found that the uterus has returned to its natural size. Since we have recognized the fact that procidentia is due to laceration of the cervix, we operate for that lesion, and have to resort to the operation for supporting the uterus by an operation on the vaginal wall in comparatively few instances, but when it becomes necessary to do it, as it is in this case, we perform it according to the method which I shall explain.

The first operation to remedy this difficulty was suggested by Marshall Hall many years ago, although I believe that neither he nor any one else, before Sims, attempted to put it into practice. Marshall Hall suggested that two parallel surfaces be denuded on the anterior wall of the vagina, and that these denuded surfaces should be brought together with sutures. In that way a double vagina would be formed. The operation proved an entire failure, for one fold after another of the anterior wall of the vagina would come down against the line of union until, if absorption did not take place and the whole line give way, the septum would be gradually dilated until it came against the posterior wall of the vagina, and the operation would thus prove a failure, and this was a question of only a few weeks.

This operation was modified by the late Dr. Sims about the year 1846. He brought these surfaces together at one end near the neck of the bladder, and passed the sutures in the same way across the undenuded portion. This was a great advance, but before going further, I should mention that Dr. Sims's first attempt was not as I have described. He was like the man who was "death on fits;" if he could get his patient to have fits, he knew where he stood. This was about the time that Dr. Sims was introducing his operation for vesico-vaginal fistula. He attempted to cure the trouble by removing the excess of vaginal tissue, and making a vesico-vaginal fistula, and then bringing the surfaces together. But he failed to enter the bladder, as he did not realize that the bladder and vagina are in contact to a very limited extent, and confined to the triangular space between the mouth of the ureters and the urethra, each side of which does not measure more than one inch. This operation, without entering the bladder, was wrong in principle, and it failed. When this tissue was crowded up against the bladder it encroached greatly upon the cavity, and pressed more or less upon the ureters, setting up irritation and cystitis, and leading to dilatation of the ureter and renal disease. I have followed up

several cases where that operation was done, and the result was as I have described; I am, therefore, sorry to see that within recent times this form of operation has again been proposed.

Having failed with this, then Dr. Sims took the Marshall Hall operation, and modified it by bringing the lower extremities of the parallel surfaces together, making the denuded surface of a V-shape. This, as I have said, was a great advance. If these surfaces were brought together in the median line, it acted as a crutch in front of the uterus to keep it up. The fold thus formed grasped the enlarged cervix and supported it, provided the tissues below met in the median line. But after the lapse of a few months, when involution had taken place, this fold no longer grasped the uterus, and it would sometimes get behind the pouch and come down as before.

Having seen these cases, where there was a great deal of trouble, I modified the operation. This consisted in making a cross-section in front of the cervix, thus uniting the extremities of the V-shaped denudation. I had no fear in shutting up the cavity, although Sims always left an opening in order that fluids might escape, and yet nothing but a little mucus could be secreted, and this would be easily absorbed. The principle is this, if you can support the uterus and allow involution to take place, the over-stretched tissue will retract, and the vagina will return to its natural size. Although there is a cavity left between the surfaces at the time of the operation, there will be no cavity when the tissues have retracted. It is like a piece of over-stretched India-rubber—if the traction is taken off, there will be retraction. The closure of this cavity formed no objection to the operation, while the opening left by Sims was rather a weak point. I thought that I had reached perfection in the operation, for when properly performed, it was all that could be asked; but I found that the method could never come into general use, because it required constant practice to bring the line of union always in the median line. If the upper stitches were not properly introduced, denuded and undenuded portions would be brought together, and the operation would fail. It required an expert to bring the parts up in such a way that they would fold on themselves instead of coming together as with a running-string. I then made the modification of the operation which I shall use to-day.

In these cases, the uterus is retroverted, and the first thing to be done is to antevert it, and then pass a probang into the cul-de-sac of the vagina, and thus keep the uterus thrown forward. Now place the patient on the left side and introduce Sims's speculum. Then find three points, one on each side of the uterus and a third in front, which can be brought together without undue traction. A surface is then denuded at these three points, and the two lateral ones are brought in apposition with the median point, and secured by an interrupted silver suture. When this suture has been twisted, a fold is brought up in front of the cervix, by which the uterus is held up. We have then two long folds formed by the excess of tissue and extending toward the vaginal outlet, and which have to be disposed of. One advantage of this method is that it dispenses with the hand of an assistant, for the uterus cannot now come down. The old operation was a bloody one, for the whole surface had to be denuded before the sutures could be introduced. In the present operation, having inserted the first suture, a portion of the crest of the fold can be denuded on each side for half an inch in extent, and readily brought together, as several sutures are introduced. Having done this, the process is continued until all of the folds have been turned in.

When there is an excess of loose tissue about the urethra, it is necessary to dispose of the mass in a special manner, as it cannot be gotten rid of by continuing the line from the cervix. If we separate in the median line, at the crest of the cystocele, and go off to each side so that the line of union on the anterior wall is χ-shape inverted, this excess of tissue can all be drawn up. But there is almost always a break in the line of union at the junction of the three lines, so that a second operation becomes necessary. To overcome this difficulty, it is better to stop turning in the tissues along the median line just at the crest of the cystocele. Then denude below two lines near the neck of the bladder, and across the vagina from one sulcus to the other, so that these surfaces represent a crescent in shape, with undenuded tissue in the centre. If these freshened surfaces have been made at the proper distance from each other, all the loose tissue about the urethra will disappear as soon as the sutures have been secured.

You must bear in mind, as I have already stated, that the whole object to be gained by any operative procedure in procidentia is to hold the uterus at such a position in the pelvis that the circulation can be restored, thus giving a chance for the involution which has been arrested to be completed. We cannot gain this object by any form of pessary yet devised, for the downward pressure will force the folds of tissue into every crevice, causing sloughing, or with the effect of dilating the vagina still more.

The success of this operation depends upon the use of just the proper amount of traction in bringing the parts together. The denuded surfaces should not be separated any more than is necessary to bring the parts in apposition. If the denudation extends any further than this, it makes no difference what kind of sutures you use, they will cut out. No suture can stand the persistent traction of the pelvic fascia. It is an art to know how far apart you should denude these surfaces.

(The patient was now brought in and operated on in the manner described.)

The operation is now completed by turning in the excess of tissue, and the anterior wall of the vagina is of the size that it was before the birth of this woman's first child. The term "narrowing the vagina" is a misnomer. You cannot narrow the vagina, and the term should never be used. This operation is not, therefore, for narrowing the vagina. The sulcus on each side of the vagina is in direct connection with the fascia and connective tissue of the pelvis. If you bring traction on these, no suture could stand the strain. You are to turn in the tissue as this woman would do by a plait in her dress if it was too large for her. You therefore do not narrow the vagina. You cannot approach the surfaces any nearer to each other than they were before the birth of the woman's first child. It is only the excess of overstretched tissue that is removed.

Let me now show you on this towel how to introduce a silver suture. Suppose that we have here a slit which I desire to bring together. Dr. Sims, to whom we are

indebted for the introduction of the silver suture, at first laid down the rule that it was absolutely necessary that the suture should be passed equidistant from the edge of the wound, for if this were not done, the denuded and undenuded tissue would be turned in together, as the sutures were twisted. This difficulty is overcome by shouldering the suture, as it is termed. Take a tenaculum and catch the edge of the wound at the point where you are to introduce your needle; then evert the edge of the wound, and see where you want to bring the needle out. You then advance the needle against the tenaculum as a point of counter-pressure until the point of the needle can be caught with the forceps; then catch the opposite side of the wound, and rotate it outward, and note where you want to introduce the needle. Then bring the needle out anywhere, so that you get it through. The next thing is to make a shoulder or sharp angle on the suture, so that the angle of each will come together at the line of union. Here is the important point where so many fail. Here is an instrument introduced by Dr. Sims, and called a shield. If the wires are passed through the slit, and twisted at a right angle to the direction of the shield, as is usually done, it is only an accident when the suture is not twisted too tight, so as to strangulate the tissues—and with failure as the rule. But if I bend the wires, now in the grasp of the forceps, over the thin edge of the slit, so that the forceps and handle of the shield lie together in the same direction, and twist the sutures until that angle formed by the two wires is no longer seen, the edges of the wound will be only just in contact. If the twisting is carried any further, the parts in the loop of the suture will be strangulated.

Next it is most necessary to make the suture lie flat. To do this lift the suture at the angles with a tenaculum as a fulcrum, and when you have bent the extremity over, make pressure downward with the same tenaculum at the point where you wish to cut the end of the suture off; it will then lie perfectly flat. I recently removed a suture which I had inserted seventeen years ago, and since then the woman has had two or three children. The suture was lying as flat as when it was inserted. It was, however, up to one side of the cervix. It is by attention to these details that success is attained. I would advise you to take a towel and practise what I have told you, until you acquire the knack of properly introducing the suture. When you are able to do this, you have acquired one-half the art of plastic surgery.

CASE II.—Gentlemen, I have here a case of double laceration of the cervix very deep, and also partial procidentia. To-day the laceration will be operated on, but the operation on the vaginal wall for procidentia if needed must be performed later, for the operation would take a long time, and even if I were to do it, you would not be able to see much. I can occupy the time much better by saying what I have to say about laceration of the cervix, and shall therefore ask Dr. A. P. Dudley, who was, a short time since, one of the house surgeons of the Woman's Hospital, to do the operation. He will do it just as I would do it.

There is a great deal to be said on the subject of laceration of the cervix. The lesion is of frequent occurrence. I have no doubt that Eve's cervix was lacerated, and since that time the cervix of nearly every woman who has borne children has been lacerated. I do not believe that a woman can give birth to her first child without more or less laceration of the cervix. It is a very important point to know when an operation is necessary. There are a great many cases of laceration of the cervix in which no interference is required. I sometimes think that almost more harm than good has been done by the operation, although in so many cases there is no operation that can take its place or accomplish so much. It is important to know under what circumstances an operation is required. I have noticed that in the cases in which interference is called for, there has been, as a rule, a bad getting up after the labor. If a woman has no milk, or does not get up well, it is always a suspicious sign of laceration of the cervix, with the occurrence of a certain amount of septic inflammation in the pelvic tissues, and it is probable that the laceration will not heal so long as that inflammation exists. I believe that if obstetricians understood this matter more thoroughly, and gave a little more personal attention to a patient who has been delivered with forceps, or who has had a hard or rapid labor, and determine whether or not pelvic inflammation exists, so that it may then be treated, in the majority of cases nature would readily repair the injury, and that within a few years the operation will be called for less frequently than it is at present. If this pelvic inflammation exists, it should receive proper treatment immediately after labor, for at this time treatment will accomplish more in a few weeks than can be gained in months, if the disease is allowed to go unchecked. The early recognition of the trouble is therefore of great importance, as the reparative process would be as active as that which would take place in the uterus under favorable circumstances.

If we have the case going on beyond that time, with involution arrested, with the uterus enlarged, and more or less pelvic inflammation or thickened broad ligaments, which is the usual lesion, the case must now be properly prepared before the cervix should be operated upon. Until the preparatory treatment has been carried out, it is impossible to say what cases require operation and what cases do not. This is where the mistake is made by many. The patient must be gotten as well as possible before an operation is performed. What you wish to do is to cure the pelvic inflammation and restore the circulation to its proper condition. So long as there is cellulitis, or any form of pelvic inflammation, the circulation must be obstructed, causing the parts to roll out and to exaggerate the existing lesion. I at one time thought that the rolling out was due to mechanical causes such as standing or walking about, but I am now satisfied that it is due to obstruction of the pelvic circulation. I was once astonished to find that a young girl, with whose history I was well acquainted, apparently had a laceration almost to the internal os. She had cellulitis, and I supposed that an abortion had been produced. I treated the cellulitis, and as the inflammation improved, the parts rolled back, and she then had a virgin os. I mention this case to show the importance of proper preparatory treatment.

How are you to treat such a case? If possible, she should be treated in a hospital. She should be taken from her home. She is unfit to be a wife, and must be in a position where she has no care or responsibility, and to vegetate, if possible, for a few months. The mind

has so much influence over the body, especially in this condition, that it is important to keep the mind tranquil. The main treatment is hot water injections, but these must be given properly. They should be given by the hand syringe and not with the siphon syringe. Heat, cold, and electricity will cause the vessels to contract and bring about the condition we desire. Electricity will cause the vessels to contract, but only while the current is passing. Cold will cause the vessels to contract, but unfortunately when reaction occurs, the vessels become larger than before. Heat at first produces a temporary congestion, but if the application is prolonged, contraction of the vessels is produced; therefore, the reaction from heat is contraction. We wish to cause contraction of these vessels and if so we can starve out the cellulitis and bring about absorption of the inflammatory deposits present. I hold that there is something in the jet of the hand syringe. It probably acts on the same principle as the dashing of water into the face of a person who has fainted. If you give a woman a douche with a siphon syringe and prolong it for an hour or two, you get about the same effect as from a fifteen minute injection with a Davidson's syringe. If the siphon syringe is used, it must be prolonged for a considerable time. The use of hot water, with the hand syringe, as the patient lies on her back with the hips elevated, has the effect of puffing out the vagina, so that the hot water comes in direct contact with the vessels, and will accomplish more than any other means of treatment.

The application of iodine will prove of the greatest benefit. This is not to be applied to the uterine canal for fear of increasing the cellulitis. I have not made an application to the uterine canal during the last five or six years, except in cases of vegetations, where I have been obliged to use it to control hemorrhage. There exists the closest relation between the surface of the uterine canal and the peritoneum. If you have an assistant, Sims's speculum may be used, with the patient on the left side so that the iodine may be applied to the posterior cul-de-sac and to as much of the vagina as possible without coming in contact with the external orifice. The preparation which I use is known as Churchill's tincture of iodine, and is four times stronger than the ordinary tincture. If you have no assistant, the woman may be placed in the knee-chest position, so that Sims's speculum can be held in one hand as the application of iodine is made with the other.

If you have the patient in the hospital under control, you can daily put a pledget of cotton, saturated with glycerine, under the well side of the uterus, behind, in front, or in such a way that the uterus shall be raised somewhat so as to take the drag off of the thickened broad ligaments. A pessary is not applicable, for it will press against one broad ligament or the other, or if the inflammation is behind the uterus, you cannot apply it at all. You make the best progress when you have the patient directly under your eye, and can daily use these pledgets of cotton so as to raise up the neck and take the strain from the shortened broad ligaments, for this does much to restore the circulation in the pelvis.

The follicles in the mucous membrane of the cervix, undergo cystic degeneration in this condition and it is very important to get rid of them. As these enlarge and increase in number, they roll out the tissues, to a point where, when cicatrization takes place, the parts are almost strangulated as if a cord were wrapped around the cervix. Everything is to be gained by puncturing these cysts. There are sometimes literally thousands of them, and just as you get rid of them by puncture you help to restore the circulation, and the parts gradually roll back. It is not until the cysts have been emptied, the flaps reduced in size, and the parts have rolled in again that you can tell what cases will require operation. You will be surprised at the amount of improvement which can be brought about in two or three months of this treatment, and its use will lessen the number of operations necessary to be performed on the cervix. If the operation is done too soon, these cysts will be shut up within the flaps as a source of irritation and if you do get the union that is desired, there will be no improvement. On the other hand, even with the uterus four or five inches deep, if the treatment has been properly carried out so that the cysts have been emptied and the cellulitis reduced, it will in the course of three or four weeks after the operation return to nearly its natural size.

I have been speaking of cases of recent origin. You will, however, meet with a class of cases which have extended over a period of years, where the woman has become anæmic and nervous, and presents any number of reflex symptoms, from a pain in the end of the nose to a pain in the toe, for which no reason can be found. She has frequent headache, pain in the back, and particularly in the nape of the neck, which is a common sympton, often sleeplessness, and inability to fix her mind on any subject, or to take any responsibility. In many of these cases, the parts have healed and often there is apparently no laceration, but the whole uterus, and especially the neck, remains too large.

If you take a case of enlargement of the uterus, from any ordinary condition, you will find, as you run your finger into the cul-de-sac of the vagina, upon the sides, that the organ is of uniform thickness, so that the portion of the cervix below the vaginal junction is of the same size as the body above. It is a uniform enlargement. If, on the other hand, you take a laceration, whether a fissure exists or not, there is always a certain amount of rolling out, so that you can feel a mushroom-shaped cervix. After the flaps have separated, nature will sometimes go to work and fill in the space between them with cicatricial tissue, or a new growth of tissue, as it has been claimed, until the fissure is filled up, and there is apparently no laceration, and yet in these cases the injury often has been the greatest. The only exception to this rule of rolling out, or club-shaped cervix, as an indication of the injury, is the circular laceration. If the lacerations take place in different directions, from within outward, so as not to reach the vaginal surface, they will open out the cervix so that the finger can be passed to the internal os. This is not an uncommon form of laceration, and there is here no rolling out as is seen in cases of lateral laceration. In the ordinary form of laceration, many think that if they simply denude on each side and bring the parts together, without removing the dense tissue in the angles, that they have overcome the difficulty, and they are surprised to find that they do not get good results. If I place a spool between my fingers, and then bring them together, I have included

a foreign body which will obstruct the circulation. Instead of the uterus decreasing in size after the operation, if this forelgn body be left, it becomes larger, and the woman, instead of being relieved, is made worse.

In these cases, where there is no apparent laceration, and the cervix is enlarged, if you take a pair of scissors and cut through the tissues down to the vaginal junction, you will pass through or come to dense tissue. Cut this tissue out, and you may call it cicatricial tissue, new growth, or whatever you choose. It is a clinical fact that, if you do thoroughly remove this tissue and bring the surfaces together, you will relieve the symptoms. An important gain is that you will also cure the anæmia. Why? You know that the sympathetic presides over the genital organs, and also over nutrition, so that the relation is a very close one; therefore, when a source of irritation has been removed, nutrition will promptly assert itself. This is the only way that we can explain the fact that, after the removal of this tissue, the anæmia will disappear, and the woman will sometimes gain twenty or thirty pounds of flesh in a year, and will, at the same time, get rid of the most anomalous reflex symptoms.

I shall now refer to circular laceration. The first step in the operation necessary for the relief of this injury, is to cut open the cervix on each side with a pair of scissors to the vaginal junction, then denude a surface over each flap, leaving a space between large enough to form the uterine canal when these have been brought together with sutures, as in an ordinary lateral laceration. In these cases you will often find an immense number of cysts about the internal os, where the mucous follicles are large, and they can be brought into view and removed, on widely separating the flaps when the parts have been drawn down to the vaginal outlet. Occasionally we are called on to treat cases of lacerated cervix which have been neglected through a long series of years, until the whole cervix has undergone cystic degeneration. I have been misrepresented as opposing amputation of the cervix in such cases. I have never opposed the operation, but have always resorted to it in a certain set of cases. It is bad surgery to shut up these cysts, and it is as impossible sometimes to bring about a healthy condition, as it is to do so in the tissues of a tonsil which has been inflamed for many years. The shortest way to effect a cure is to get rid of the whole diseased tissue. In these cases, I partially amputate the cervix, removing only the diseased tissue with a pair of scissors, and always cover the stump with vaginal tissue by Sims's method. My objection to amputation does not apply to these cases, but to the condition which is claimed to be an elongated cervix. I deny that the flap of a laceration is elongated, even if it appears to be so. I will show you where the explanation of the condition lies, and also how you can cure these cases without amputation. Sometimes where the vaginal canal is very open, the anterior lip will roll out, and the vaginal tissues will become crowded over it, so that the lip will be apparently much longer than the posterior one, but the amount of uterine structure in one is no more than that in the other lip. The appearance is due simply to the crowding down of the vaginal tissues, as in a prepuce. This is proven by the introduction of a sound into the bladder, when the extremity of the instrument reaches a point on the crown of the cervix, which it could not do if its insertion within the vagina was really where it seemed to be. Both flaps can be brought together accurately, by so denuding the vaginal surface, when forming the angle, that one flap shall be as long as the other, and this can be done without any reference to the angle at the uterine canal. If done properly, the tissues on the anterior lip will roll in, or be stripped back, so that the anterior lip will be no longer than the posterior. In these cases I have opposed amputation because I know that the neck is not elongated. There are cases which are termed congenital elongation of the neck of the womb, but these have nothing to do with the condition which I have been describing.

The patient on whom Dr. Dudley is operating has another lesion, which may be described by saying that the running string has been lost from the vaginal entrance. This is what is usually called a laceration of the perineum. Although we shall not have time to operate, I should like to explain the condition. As a rule, the perineal body is not torn by the child's head. It may be torn by the forceps from above downward. This lesion of the perineum is very different from what it is supposed to be. It is a tear directly across the passage and just within the vagina behind the fourchette. The idea is, that when the child's head is pressed against the muscular barrier, and the outlet of the vagina is not dilated enough for it to pass, the canal must be torn at its insertion, if the force behind is able to overcome the resistance in front. It is like a strong man trying to go through a door which is not large enough to permit him to pass through, and, in his efforts, he may carry the doorframe with him; but the force could not be so applied that he would simply break the sill across. The perineum is generally torn when the sphincter is, and when the tear begins and extends from before backward in the median line. The relaxation and protrusion of the tissues, in this case, are due to the transverse tear I have described, and the cicatricial line can be plainly seen. It has separated the pelvic fascia just where it is reflected from the sulcus on each side over the muscles at the vaginal outlet. In no case do the fibres of the levator ani muscle interlace, and often they do not approach within half an inch of each other. When the transverse tear takes place, and the head continues to advance, the connective tissue between these muscles separates, and the muscles themselves are drawn aside, as a curtain, with often not the slightest injury to the skin in front, or to the mucous coat of the vaginal canal. The transverse perineal muscle, from its insertion in the tuber ischii behind, must naturally tend to draw the muscles apart and roll out the tissues, as the lower lip would fall off from the bone if the attachment in front were cut away. In other words, the real extent of injury is an exaggeration. It is very rare that any portion of the skin, or any tissue outside of the vaginal outlet, is torn in these cases.

What is the effect of this? The woman does not suffer from want of support to the uterus when she has what is called a laceration of the perineum. The perineum has nothing to do with its support. A woman's perineum is certainly lacerated when the sphincter is torn through the median line, and yet I have to see the first woman who has complained of difficulty in walking or standing from this cause. That, of itself, shows that it is not the perineum that supports the uterus.

The purpose of the perineum is to support the curve of the rectum and prevent the rectum from bulging into the vagina. When the muscles are separated in these cases, we must have a rectocele. The sulcus on each side of the vagina is in direct connection with the connective tissue of the pelvis. When the parts are normal the upper portion of the vagina is as fixed as the roof of this room. The fascia and connective tissue on each side bring the posterior wall up against the anterior one. It is like taking a rubber tube and making lateral traction: the sides will come together. I claim that separation takes place, across the vagina, behind the outlet, where the vagina is inserted into the muscles. When this occurs, the fascia on each side is divided. As soon as that is done, there is a slacking of all the connective tissue in the pelvis and the woman suffers not from a want of support to the uterus from loss of the perineum, but from want of proper support to the blood-vessels, which, without the needed support, will enlarge indefinitely. After a long time, when the uterus has become heavy from continued congestion, it comes down to the floor of the pelvis, becomes retroverted, and so on, but this is not the case in the beginning. I cannot explain this slacking up of the pelvic fascia better than by describing something that occurred to me when a boy, in the days of trundle-beds. With the springs of to-day you know nothing about the manner in which the sacking-cloth was stretched on these beds. The bed in which the children slept was pushed under a larger one, and the bedding was supported by a piece of canvas or sacking, which was kept tight by means of ropes secured to pins in the frame of the bed. By long usage, these pegs had become worn at the bottom, and I well recollect the time we had when the rope would slip off trom the pins. When several loops of the rope slipped at the foot of the bed, the sacking would of course become slack up the middle. Suppose the rope could not be put on the pins again, with the condition I have drawn here on the board, could we not overcome the difficulty by bringing up the slack in a fold on each side across the lower angles of the bed? This fold is just in the same direction and relatively in the same position where we bring up the tissues in the vagina, with the object of making traction on the fascia and connective tissue of the pelvis, that the proper support may again be given to the rectum and bloodvessels. To do this, and on the same principle as I did in the other operation on the anterior wall, to dispose of the excess of tissue about the urethra, you are to denude a crescent with a horn extending into the sulcus on each side. This denuded surface will, therefore, extend across the vagina, and just within its entrance, and will include the anterior face of the rectocele and the posterior face of the muscular barrier on each side of the vaginal outlet. These two surfaces are to be brought together by interrupted sutures, as was done on the anterior wall, and we thus dispose of the rectocele. The separated muscles are then to be brought together by special sutures, but it is impossible to explain the operation so that you can understand it fully, without demonstrating it on the subject.

I will state, that after you have been graduated I will be glad to see any of you at the Woman's Hospital in New York. My operating day is Tuesday, at two o'clock. I will then be able to show you every step of the operation.

ORIGINAL ARTICLES.

FOURTEEN DAYS WITH PASTEUR.

BY FRANK S. BILLINGS, D.V.M.,
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THE writer was selected by Dr. H. C. H. Herold, President of the Board of Health of Newark, N. J., and by Dr. Wm. O'Gorman, of that city, to accompany to Paris four boys who had been bitten by a *supposed* rabid dog, in order that they might have the advantage of the preventive treatment of M. Pasteur. As the parents of the children had not sufficient means to send them, Dr. O'Gorman, a gentleman of great benevolence, and enjoying the first position among the practitioners of Newark, resolved that they should be sent, and opened a public subscription which was liberally supported by many benevolent citizens of that city and elsewhere.

The names of these children were, William Lane, fourteen; Austin Fitzgerald, ten; Patrick Reynolds, ten; and Eddie Ryan, five years old. They were bitten on Dec. 2, 1885, and all by the same dog. The wounds of Lane and Reynolds were upon the hands, and under ordinary circumstances would be thought nothing of, but, as is well known, the smallest wounds in case of rabies are even more to be feared than large ones that bleed freely, and again that the percentage of rabies in human beings actually resulting from the bite of a rabid dog has been found to be greater for the superior extremities and the face than for other parts of the body, on account of their being generally unprotected by clothing. The little Ryan boy was bitten quite deeply in the elbow of the right arm, probably by the teeth of the dog, but at the same time the teeth had to penetrate through an unusually thick and felt-like piece of cloth with which the arm of his winter coat happened to be patched at this point. The Fitzgerald boy was not only the most delicate of the four, but was also the most severely bitten; one wound being upon the hand, the thumb having been penetrated very deeply at its base. The other bite was in the gluteal region, and was accompanied by considerable laceration and free bleeding; naturally, the teeth had penetrated through clothing also. Two other children were also bitten by the same dog, but as they were not sent to Paris and were never seen by the writer, but little can be said about them; still, as the whole escapade was an experiment, it is very well that they were left behind as controls, but we must earnestly hope they are in no danger thereby. At about the same time of the day these children were attacked, the same dog was said to have bitten several other dogs, six of which have been discovered and are still in close confinement under veterinary supervision, by order of the Newark Board of Health.

It is greatly to be regretted that so much of hypothesis is connected with these cases as regards the true nature of the trouble with the dog. Indeed, so much contradiction exists, that one can scarcely say what kind of a dog it was, but from all that can be culled from these stories, it seems that it was a middle-sized, short-haired dog, probably with some pointer in his breeding.

The following facts tend to support the hypothesis of rabies:

The dog was certainly in a state of the most intense cerebral excitement. Unlike an ugly, biting dog, he did not stop to tear up or continually bite, or fight with either the children or with the dogs bitten by him. In his frenzy he simply snapped at such living objects as were directly in his way, making a quick snapping bite or two, scarcely stopping to shake the children at all, and then making off; it was not long before he was followed by a small army of would-be-destroyers, and was finally found in a back yard on a shed, trying to gnaw his way through the surrounding board fence. There he was shot, and found his way to the knackers without any autopsy having been made.

This history, however, is sufficient to have warranted the Newark authorities looking upon the case with the utmost suspicion, especially as there had been several genuine cases of rabies canina and hydrophobia in the city and vicinity a short time previously. One was especially instructive, as it illustrates the circumspection with which sympathetic people should avoid demonstrations to stray and suffering curs upon the streets. The lady in question, on passing down a street, saw a small dog by the curb in apparent suffering; she stopped to pat and sympathize with it; the dog snapped at her, merely drawing a little blood from the hand; she died under terrible suffering within two months from that day. The writer knows of two similar cases in the vicinity of Newark, and there had been quite an unusual number of suspicious dogs killed before this accident.

There can be but little sympathy from scientists with the excitement kept up by the papers; still some good will come of it, and, as some assert, only by such a course is it possible to attain much support from the public for the advance of preventive medicine.

The children sailed on the Steamship "Canada" of the Companie Générale Transatlantique, on Wednesday, December 9, 1885, the Company also taking a liberal part in the general benevolence by placing them in the second cabin both to and from Paris. They were treated with great consideration, and all of them showed most pleasing indications of physical improvement. The passage over, as well as the return upon the same ship, was wonderfully pleasant. The "Canada" arrived at Havre December 21st, the passengers being directly transferred to the cars of a special train for Paris, where they arrived at 4 P. M. It is but a matter of simple justice to state that the expedition with which the children were placed under treatment, as well as the saving of the writer and them from any amount of extortion, was largely due to the kind and continued attention of Mr. J. H. Haynie, whose very interesting and complete letters have appeared as from the "Special Paris Correspondent," in the *New York Times*.

The children were at once taken to a hotel, from which they were subsequently removed in order to give them every comfort and care possible. The writer immediately proceeded to M. Pasteur's laboratory, and induced him to place them under treatment that very evening, for it seemed in this case that "time was (not only) money," but might be life as well. Not being a physician, and the operation involved being a surgical one, M. Pasteur does not do it, but has it done by Dr. Grancher, Professor in the University, and the most noted clinician in children's diseases of the present French school. It is scarcely necessary to say that Grancher, like Pasteur, gives his services free of all remuneration so far as this treatment is concerned, save the grand satisfaction of devotion to the salvation of the race from one of the most terrible diseases known to man.

The inoculation, or rather the introduction of the preventive material, is done with the ordinary hypodermatic syringe, the point of inoculation being the umbilical region. The operation is never done on the same side two days in succession. For children under twelve or fourteen years old one-half a syringeful of the fluid is used, for adults nearly the entire contents. The instrument is never emptied completely, one division of the piston being always left. This is probably done so as not to inject the thickest part of the contents, for the material is not filtered. One would expect to see some irritative reaction following the injections, but this does not generally result in the case of the first five or six, but with the last two or three, especially the very last, when the most active virus is used, a distinct hyperæmia at the point of inoculation resulted, which disappears in the course of forty-eight hours. No disturbance of the functional activities followed in any of the children. Whether or not any augmentation in the temperature occurred it was impossible to ascertain from the unsatisfactory hotel conditions under which they were first placed. The number of inoculations varies in general from ten to fourteen, according to the age of the patient and the irritative reaction following the introduction of the stronger virus toward the end of the treatment.

The treatment began December 21, 1885, and ended January 1, 1886; the children were, therefore, inoculated *eleven* times, with the exception of the youngest, Eddie Ryan, who received the virus but *ten* times.

The children sailed from Havre on the evening of January 2, and arrived in New York January 14, 1886, during the whole of which time they were perfectly well.

M. PASTEUR'S DISCOVERIES.

Pasteur assured me that he had never been able to isolate, cultivate, and produce a microbic virus capable of producing rabies in any animal. He assumes that a specific microbe must be present, but neither he nor his extremely able assistant, Dr. Roux, has been able to prove its existence. It seems to me to be one of the failings of Pasteur to have no confidence in the correctness of any work done by others except in his own laboratory. While in Paris a communication was received at the Academy to the effect that Fol, of Geneva, had succeeded in staining a specific coccus in the cerebral tissues of rabid animals, and had cultivated it and produced rabies with it. Whether or not this is correct, I have no means of judging, not having seen the original communication

from Fol, but it was at once asserted to be "impossible" in the laboratory of M. Pasteur, simply because they had not been successful in the same direction.

Such things do not deteriorate from the grandeur of the work already done, which began with the study of the ferments with reference to beer and vinegar, followed by the researches upon the silkworm disease, which have resulted in saving millions of dollars to France. Pasteur next turned his attention to some of the contagious diseases of animals, the first being fowl cholera, then anthrax, rouget, and finally rabies. The first three of these are known to be due to specific microbes. The work of Pasteur has not, however, been that of a bacteriologist in the strict sense, but, filled with the idea that a preventive virus could be made from the elements of such diseased organisms, he has worked more from the methodical standpoint of the chemist than from that of the bacteriologist. Still, with anthrax and fowl cholera he has shown that a virus, or rather two, can be obtained capable of producing a period of immunity for at least one year (anthrax), and for hog cholera, a disease of far more interest to us in America, he has also shown that a virus can be prepared which will give the inoculated animals a certain degree of immunity. With reference, however, to hog cholera, the results are by no means so satisfactory at present as in anthrax. There seems to be a far greater difference in the susceptibility among the different breeds of hogs, with regard to the virus, than in cattle with anthrax. Much may be done in this field yet, for the work of experimentation has ceased at Pasteur's laboratory since the death of the lamented Thuillier, the whole time of those laboring having been given to the study of rabies.

Pasteur seems to have found that every virus *may* be rendered a vaccine under some peculiar manner of treatment; such as the continued exposure to a certain degree of heat for a given time, or to the action of the air, or to desiccation. Bacillus anthracis, for instance, develops spores at a temperature of 37° C., but at over 40° this spore development ceases, there being a very rapid development of very short rods by fission. It has been found that if a spore cultivation of bacillus anthracis is sown in bouillon placed in a thermostat at 43° C., and kept clear, that, notwithstanding the fact that a development of rods occurs, the fluid gradually loses its virulence until, at about the fifteenth day, it causes no effect upon an animal that it would at first kill. Such a bouillon constitutes a primary vaccine against anthrax; one that has been exposed to the same degree of heat for about nine days constitutes a secondary, and a freshly sowed one of one day old at a still lower temperature, constitutes the primary vaccine.

Now here comes the peculiarity of all these artificial vaccines which constitutes fully as great a discovery on the part of M. Pasteur, as that of their practical application, viz.: Each virus retains the same virulency so long as we continue making artificial cultivations in bouillon from it. Having established the fact that a bouillon cultivation of bacillus anthracis had either a negative or very weak action after exposure to a heat of 43° C. for fifteen days, it has been found that, with due precautions against adventitious pollution, we can go on and seed bouillon flocks indefinitely from the primary one, and always have a virus of the same degree of activity. The same is true of the secondary, or stronger, and the tertiary, or strongest vaccine virus. On the contrary, if we take a rabbit and inoculate with this primary or weak vaccine virus, and then go on directly from one rabbit to another, it has been found that in course of time we again arrive at a material having all the characteristics and virulency of the original and natural material.

Another singular fact has been established by Pasteur, with regard to the etiological elements of several diseases. In rabies he has found that when the same material from a rabid dog is carried through monkeys it soon loses its virulency, while if carried through rabbits its activity is augmented. We shall return to this subject immediately. In hog cholera, the infectious elements become ameliorated in virulency when passed through rabbits, but augment when passed through pigeons.

Here, again, we come to the same rule: an artificial cultivation made in bouillon from one of these rabbits or pigeons which killed at a certain number of days, retains that characteristic so long as such cultivations are made, but the moment we again carry it through a series of either animal, it soon follows the course which has been found peculiar to the species used.

I will close this part of the subject with a few remarks of the impression made upon me by M. Pasteur, and about his laboratory, and then return to the consideration of the preparation of the vaccine material used in his anti-rabic inoculations.

M. Pasteur is sixty-three years old. He is a short, thick-set person, about five feet six or seven inches high, with straight black hair, a little sprinkled with gray, and short-trimmed whiskers of the same character. Some few years since, he had an attack of paralysis which affected his left side somewhat, so that the movements of that side are not as free as those of the other. In some ways he reminds one of the late General Grant. He is not much of a talker; has in general rather unemotional features, but has a pair of kindly, dark eyes, with heavy eyebrows. From casual observation or without sufficient acquaintance, under peculiar conditions, one would never be led to think that a man so apparently unemotional possessed a tenderness and emotional sympathy far beyond that of ordinary women. When babes or very young children displayed exceeding fright after the first inoculation, when brought each succeeding day, one did not have to watch very closely to see the pearls of sympathy gather in the master's eyes.

It would be unjust to Pasteur not to enter our earnest protest against an impression that has gained more or less extension over the world, viz., that he is exclusive, that he does not publish his methods and keeps them only for himself and those connected with him in work.

In this regard Pasteur's position has been exactly what Koch's was when connected with the *Reich's*

Gesundheits Amt. Neither of them was in the position of teacher; the former had his laboratory and government aid that he might proceed in a certain line of research which, he had become convinced, would be of immense value to the agricultural interests of the country. Communications have been occasionally made to the Academy, as circumstances warranted, regarding the nature of the work and how it has been done, but it was not Pasteur's mission to teach, and it appears as if he had never had sufficient leisure to write any extended works. But, as he has himself said, "it is all in the *Comptes Rendus*," even to rabies. The same is true of Koch. While in the position mentioned, he was appointed to do certain work in connection with given diseases. The German Government, recognizing the demands of the scientific world, promoted Koch to a professorship in the Berlin University, with the probable result that while an inferior man could have equally well served in that position, the world will suffer in the interference thus made in the work of one of its most noted investigators.

Pasteur, while not the author of vaccination, may be truly styled that of the Vaccine School of Research. Another, which will divide the honors with it, is known as the Bacterial School. Both of them are engaged in the work of Preventive Medicine. The school established by Pasteur is certainly limited in its practical application, while the other, which is more of a Teutonic production, is almost limitless in the application to the prevention of disease. The one is direct in its application, the other indirect. The one is simply practical, the other both practical and philosophical, for the proper application of the teachings of bacteriology to the needs of man, requires the most exact study of all the various phases of sociology, climatology, etc.

THE LABORATORY.

The laboratory of M. Pasteur is situated upon the grounds of École Normale, the entrance being No. 45 Rue d'Ulm. It is a one-story building about one hundred feet long, with cellars under its entire length. One is surprised at the number of rooms, closets, etc., that it contains. In a small wing on the left is the entrance, to the left of which is the private room of the savant. To a person who has worked in the modern laboratories of Germany, that of Pasteur appears to be very parsimonious in its fittings. In fact, no better evidence is needed to support the assertion previously made, that the work done here is more from the chemical point of view than from the bacteriological, than the sparsity of paraphernalia used in the cultivation and study of bacteria. On the other hand, this very fact only goes to place more strongly in view the wonderful character of the presiding genius.

The one great medium of culture here is bouillon. I could see but little evidence which tended to prove that much attention was given to the study of bacteria from the biological point of view and their relation to, or, better, deportment in different culture media. On the other hand, such are the delicacy and exactness of technique that have been acquired by the assistants of Pasteur, that they may soon arrive at comparatively pure cultures in fluid media. As just stated, bouillon is used for almost everything, even where other persons would resort to distilled water, for there are no conveniences here for making the latter.

Pasteur and Chamberland have perfected the most wonderfully perfect clay filtering tubes I ever saw. By the use of these, under condensed air pressure, they can at once prepare water, bouillon, blood-serum, or even saccharine solutions, and have them entirely free from germs or spores of any kind, without recourse to any further sterilization by means of heat; years of experience have proven the correctness of this assertion. The saving of labor thereby, as well as time, is something that cannot well be calculated. These filters should be introduced into this country for domestic use as well as laboratory work, as I am sure that American ingenuity has never produced anything equal to them.[1]

On the ground floor we find first a photographing-room, which is used exclusively by the first assistant, Dr. Roux. Of Roux's photographs of bacteria, no person can have any conception who has not seen them. They are simply wonders of exactness, as much superior in definition and outline to those of other micro-photographers as the diamond is to charcoal. Roux is an entirely different kind of man from Pasteur. The latter is reserved, and one looks in vain for any indication of enthusiasm, in place of which he sees dogged determination. Roux, on the contrary, is one bunch of nervous energy, with no lack of perseverance marked in his countenance. He is a tall, slim fellow, who shows the indications of hard work and overtension in every expression and movement. The grasp of his hand at once expresses to you earnestness and sympathy, and if he finds you educated and interested, there is nothing that he will not do to make clear everything you desire to know.

The second assistant is a graduate of the École Normale, a M. Wasserzug. The name would indicate a German, but he would deny it, and tell you he was an Alsace Frenchman, yet I was pleased to find him utterly free from any dislike of his German neighbors—in fact, he spoke of them with the greatest earnestness, and was especially desirous of hearing everything possible of Robert Koch and his methods. He is a young man, more German than French in energy and character, and it was mostly through his personal attention that I obtained the knowledge I gathered at this laboratory.

As we leave the photographing-room, we pass into one in which we find the monkeys which have been inoculated, and thereby attained immunity from rabies. From their friendliness and good health, one would never realize that each of them had been put through a most trying ordeal, for they have all been inoculated with the most virulent rabid virus. The next room is used for all sorts of operations upon animals. Then comes a hallway from which we go down to the cellar, and also from it into the main laboratory. One of the most striking things one sees is the immense variety of flasks, tubes, balloons, etc., of glass, with tubes extending from them, and necks twisted into almost every imaginable shape. This certainly

[1] See THE MEDICAL NEWS, June 6, 1885, p. 627.

indicates that such experimentation to find the most favorable forms of vessel that can be used to prevent pollution, when the contents are being inoculated, must be very expensive. So it would be were these things all ordered from a glass factory, but here we find a most practical idea, which we should follow here if we ever have laboratories for the same purpose—*i. e.*, one of the servants of M. Pasteur is a glass-blower, and makes almost, if not all, this kind of material used, certainly all of the smaller and more complicated varieties. The light in this building is as perfect as any one could wish, as the rooms have large windows, and the main room has them on both sides.

In the cellar we find a room for the rabbits that are used in producing the rabid virus only; they are all kept in small wire cages, two in each. Artificial rabies in rabbits is not characterized by any of the ferocious symptoms which we have learned to dread in the dog. They do not appear to labor under any cerebral excitement whatever; they do not bite or fight, but simply suffer from a slowly progressing paralysis. Whether they have tetanus or not I do not know, but certainly they do not fall into any spasms on disturbance. In another room we find animals used in the study of anthrax, and so on for hog cholera, etc. As I left, Dr. Roux was beginning the study of material from the so-called enzoötic abortion in cows, a disease which causes much loss to cattlemen all over the world.

The dogs were formerly kept here, but becoming "disturbers of the peace" they have been removed to an especial building on a neighboring street. They are all kept in very securely made iron cages. Here is a fine opportunity to study canine rabies in all its stages and varieties. Upon the same grounds is situated the building in which is manufactured the anti-anthrax virus, which is being called for all over Europe.

One of the best points to be followed, which can be seen here, is, that each servant has but one kind of work to do. The assistants have nothing whatever to do with the preparation of any kind of virus after the methodic has once been completed to Pasteur's satisfaction. They are continually employed in completing some process or opening up new work. One man inoculates all the rabbits for rabies and prepares all the virus to be used in the inoculations; while another makes anthrax vaccine, and another the cultures of hog cholera. I have never seen laboratory work so well systematized or go on with so little jarring, and few men in this country have had better opportunities for judging.

What supports all this? The government of France! For this purpose M. Pasteur at present receives 50,000 francs per year, but from all that I have seen it is very doubtful if this amount fully covers the expenditures, as he is a person who would never stop to think of his own pocket when more means were necessary to arrive at the completion of some object by which his country's interests would be advanced or human suffering lessened.

THE PREPARATION OF THE VIRUS.

Pasteur's studies upon rabies began in the fall of 1882. The first human being to be inoculated was the boy Joseph Meister, on the 7th of July, 1885. The real grandeur of this work cannot well be expressed in language. The intense responsibility, the almost death-like pressure which must have weighed upon the brain of Louis Pasteur when he made this, his first experiment upon a human being, can only be appreciated by one who has become acquainted with canine rabies in its terrible violence, and hydrophobia in its unspeakable tortures, and further with the entire details of this preventive treatment, how it has been arrived at; such a person, I say, is the only one that can appreciate all Louis Pasteur has been through.

I do not know that it would not be possible to have brought home some of Pasteur's material; it could certainly have been done by the aid of rabbits on board the ship, but I did not ask for it for just one reason, and that is, that I do not consider either myself or any other person would be justified in trying to inoculate his own species, before he had himself gone through the entire work by which Pasteur has arrived at his present confidence, and by similar and repeated experiments upon dogs, have become as well acquainted with the action of his own material as Pasteur is with his.

It is well known that the saliva of the dog has always been looked upon as the essential medium of infection. It is equally well known to every practical bacteriologist, that no more difficult substance could be taken from which to isolate a specific microbe than this saliva, especially, as is probably the case, if this microbe be a coccus. From his observations of clinical phenomena, Pasteur concludes that the specific elements of rabies are to be found in a pure condition in the material of the great nervous centres, brain, and spinal cord, more easily than elsewhere. Experience has proven the correctness of this conclusion, but it still seems doubtful if we shall not also find the same elements in an equally pure condition in the large salivary glands, on the side and under the jaws, as in the brain. The very fact that this saliva is the usual medium of communication of the disease to other animals indicates this.

I have been told that for his cultivation of the microbe, which he claims to be specific, Fol used the fluid he could squeeze out from the fresh brains of sheep. To my mind, the best and most natural way to obtain a medium for the cultivation of the specific microbe in this case, if one thought he had found it, would be to dissect out the parotid duct on each side of the jaw of a large dog, and have its secretion received in appropriate vessels. I would also use cattle for the same purpose. This material can be sterilized in its natural condition, and also made into a solid medium by the use of gelatine.

However, Pasteur has found the brain-substance to answer every practical purpose; from it he prepares an artificial virus, without any direct knowledge of the existence of a specific bacterium, as is the case in the vaccine we obtain from calves. Looking upon the brain as the chief seat of this material, it was but natural that he should also select that organ as the point of inoculation for his future experiments. The supposition proved correct. Every dog thus in-

oculated from the brain of another rabid dog died in an average period of fourteen days. This fact established, the next step was to find a means of mitigating this material, and a method of using it as a preventive medium.

Not having isolated any specific bacterium, it is evident that the means used in the preparation of the anthrax virus would not be available. Hog cholera had, however, given another indication, which, if followed, might lead to valuable results—*i. e.*, the fluid elements of that disease taken directly from a sick hog, one that had been dead but a short time, if carried through rabbits loses its virulence, but increased in the same when carried through pigeons. It was but natural that Pasteur should desire to try this cerebral extract upon monkeys, as next in kin to man. We can well imagine his astonishment at finding that even the most rabid virus caused no serious disturbance, and that if the inoculations were made through a very short series of monkeys the virus becomes powerless, even upon dogs. Now here he sat upon the apex of a most difficult horn of dilemma. Here was, indeed, a means of preparing a primary rabid vaccine, but where on earth were the monkeys to come from?

This method, self-evidently, had to be given up. Had he only had the microbe isolated and cultivated, and produced this monkey rabies with it, he would have found his preventive virus much sooner than he did.

The monkey not being available, it was but natural that he should turn to that invariable object of vicarious experimental sacrifice, the rabbit. Here he was again struck by a very singular phenomenon. After experimenting for some two years, and using about 2000 rabbits, he satisfied himself that if one inoculates a primary rabbit in the brain with similar substances from a rabid dog, that that first rabbit becomes rabid in just the same time a dog would if treated in a similar manner; the next thing was to discover what would be the result of carrying such a material from rabbit to rabbit in the same manner as had been done with monkeys. This being done with innumerable series of rabbits, it was found that after thus inoculating from twenty to twenty-five rabbits that the rabid material had so degenerated in virulence that it killed each rabbit in seven days. This same material acted in the same manner when inoculated upon dogs. Hence Pasteur could almost have claimed to have produced a new disease out of an old one. Now comes the question, How can this material be diminished in virulence?

The brain-substance could not be handled very well, so that the next step was to try the spinal cord, the most accessible portion being the medulla oblongata. This was found to act exactly as the cerebral substance had done. The next step was to try the effects of heat and cold upon it in a dry atmosphere; the microbes, if present, must thus be deprived of one of the chief essentials to their existence. The dry air was easily found by placing lumps of desiccated caustic potash in the bottom of glass jars. The corks were made to fit very neatly, and were of the best quality. The medullas were then removed with the utmost care to avoid pollution with any foreign material, and hung by a thread attached to the corks in the jars.

The following results ensued: When kept in a cool place, the medullas retained their virulence a long time, which could be increased by filling the glass jars with carbonic acid gas. If, on the contrary, they were kept in a room with a constant temperature of 20° C., it was found that they lost in virulence with each day's action of the dry atmosphere and the surrounding heat, until after, or on, the expiration of fourteen days, a medulla was found which produced no perceptible action upon rabbits or dogs when introduced into the brain.

It was now necessary to discover if each one of these medullæ possessed any constancy of action according to the time they had been thus treated. One can hardly realize the amount of work that had to be done, and of the inoculations that were made before this fact could be considered as established. Finally, it again resulted as in other vaccines under methods of treatment: *that any number of medullæ of approximately the same thickness had the same action upon animals, if introduced into the brain, after having had the same amount of exposure—i. e.*, a canine medulla would cause rabies in a rabbit in 14 days; a super-virulent rabbit medulla would cause rabies in a rabbit in 7 days; after the latter had been taken out and subjected to treatment for about two weeks, all action disappeared. A slight action followed the use of one of 12 days, more of one of 10, and so on more and more until one of 1, 3, 4, or 5 became more and more virulent.

It must be borne in mind that these results were all obtained by inoculating a single animal from a given cord of a certain number of days' treatment.

The next thing to do was to try the effect of the serial introduction each day, or at intervals of a day or two, of a series of three separately treated medullæ from rabbits, beginning with a cord that had been treated for 14 days, and ending with one freshly removed from a rabid animal.

What was the result?

I should like to know what Pasteur's feelings were at the moment when he had found that no rabies followed in his animals. No man can portray them!

Then came the revelation before the Academy; the doubts and sneers; the appointing of a committee to watch and prove: the treatment of twenty-five dogs as a test case; the control experiment with twenty-five others that had not been treated to the bites of rabid dogs; the anxious waiting for time to elapse. Then Victory!

The curtain should be dropped here and the story ended, but it is not. Then followed experiment after experiment until all was as safe as the simplest injection of a harmless material, and then came the tests.

It will be seen from the following statement that the child was daily inoculated with a piece of spinal cord from a rabbit of ever-increasing virulence. The cords were subjected to the action of a dry atmosphere a shorter and shorter period after their removal from the rabbit, until finally a cord comparatively fresh—that is, only one day old—was used.

The boy Joseph Meister. The inoculations began on the 7th of July, as follows:

Date.	Time of inoculation.	Date upon which rabbit was inoculated from which the spinal cord was taken.	No. of days the cord was dried after removal.
July 7	9 A.M.	June 23, 1885	14
" 7	6 P.M.	" 25, "	12
" 8	9 A.M.	" 27, "	11
" 8	6 P.M.	" 29, "	9
" 9	11 A.M.	July 1, "	8
" 10	11 "	" 3, "	6
" 11	11 "	" 5, "	7
" 12	11 "	" 7, "	5
" 13	11 "	" 9, "	4
" 14	11 "	" 11, "	3
" 15	11 "	" 13, "	2
" 16	11 "	" 15, "	1

The method of inoculating rabbits is simply that of ordinary trephining, the instrument being smaller and having the diameter of an ordinary leadpencil.

The fresh medulla is rubbed off with bouillon, as later described, and about two divisions of the piston of an ordinary hypodermatic syringe of fluid injected under the meninges; the end of the kneedle must be curved. The skin is then sutured as in any other case.

At the same time the child was inoculated, another rabbit was immediately afterward inoculated with exactly the same material used on the child. This is done with the virus used on every person inoculated by M. Pasteur in order to keep the experiments under the most exact control. The result is thus reported by M. Pasteur:

The medullas or cords which were removed from rabbits and inoculated on the child July 11, 12, 14, 15, and 16 respectively—that is, which had been desiccated seven, five, four, three, two and one day—all gave positive results when reinoculated upon rabbits in a degree corresponding to the time they had been dried—*i. e.*, the freshest cords caused the appearance of rabies in the rabbits in seven and eight days, while the others gave results later and the driest gave none.

The fresh cords having therefore caused the outbreak of rabies in the regular time, which the most virulent cords from rabbits had previously done, and the same not having caused any disturbance whatever to appear in the boy Joseph Meister, Pasteur concludes "that the boy has not only escaped a future outbreak of canine rabies, but also that the systematic inoculation has so prepared the elements of his body that they were enabled to resist the action of still more virulent rabbit rabies."

This virus from each of the cords is made by taking a small piece about one-fourth of an inch in length and rubbing it up with a glass rod and adding sterilized bouillon, drop by drop, until the fluid and mass are well mixed, and about a tablespoonful of bouillon added. It is not filtered, and some of the substance necessarily settles to the bottom of the glass as a sediment.

The virus is made fresh every day, the glasses containing it being prepared about 10 A.M., the inoculation occurring about 11 A.M. While waiting, the glasses are carefully covered so as to prevent anything falling into them from the air. As no cord is used a second time (that is, as a cord which if used to day would be one that had dried six days, but which if put back again into the bottle would be a seven-day cord, and so on), it is evident that a number of rabbits must be inoculated daily in order to keep up the supply of very virulent cords, according to the number of patients present, and also according to the period of their treatment; newly arrived patients requiring cords that have been dried fourteen days, and older ones those of corresponding freshness.

I have thus endeavored to place Pasteur's method as fully before the readers of THE NEWS as possible. It it easily to be seen that the preparation of such material is not a matter that can be done either by private practitioners, or even at ordinary hospitals.

SOME PRACTICAL APPLICATIONS OF PASTEUR'S METHODS.

Muzzling dogs is powerless to prevent the extension of rabies among them.

The best way to prevent rabies in man is to prevent it in dogs. This can be done in the following manner:

1st. Each State should have a State Laboratory in connection with its Board of Health, in one department of which such cords should be constantly prepared, and the physicians and veterinarians of the State educated in their use on animals.

Every endeavor must be made to discover the microbe of this disease, and an artificial virus from the same, which can be used in the same manner and as trustworthy as that at present used by Pasteur.

2d. The next step is to discover, by direct experiment, how long the immunity from rabies can be trusted to continue in dogs after they have been treated as above described.

3d. Laws must be made conformable to the above, and record books kept in each city. Every dog must not only be inoculated but licensed, certificates of which must be filed with the proper authorities, and presented each year when the new license is applied for.

Experience only can tell the time in which such certificates must expire and be renewed.

4th. All dogs not inoculated must be killed at once.

This is practicable; and only in this way can we do away with canine rabies, and eventually remove all danger of hydrophobia from our citizens.

NEW YORK, January 20, 1886.

IMPACTED GALLSTONE IN THE DUCTUS CHOLEDOCHUS.[1]

BY F. LANGE, M.D.,
OF NEW YORK.

ABOUT the middle of July Dr. Heppenheimer, of this city, sent to me Mrs. B., 37 years of age, married. A previously healthy woman, about six months ago she commenced to suffer with severe pain in her right side, over the region of the liver. Sometimes she suffered so severely that she was forced to cry out. The pain was also located toward her back.

[1] Read before the New York Surgical Society, Dec. 22, 1885.

About one month later jaundice set in, and has since remained. Loss of appetite, vomiting, mostly in the morning, and sometimes of a greenish material. The constant pain has weakened her very much, though her nutrition is not so much impaired as one would expect after such protracted and severe suffering. Her color is of a deep greenish-yellow; skin flabby. The expression of her face I should describe as languid and dissatisfied. She is feverish, and often feels chilly, but has never had a marked rigor; stools light in color; bowels tend to constipation; urine icteric; constant pain in her back; sometimes pain in epigastric region; entire loss of appetite; often nausea. As the principal result of physical examination an enlarged liver presented itself. Fulness from fourth intercostal space down to one inch above umbilicus, and from that point obliquely down to the anterior superior spinous process of the ilium the left lobe seems to be proportionally much enlarged. Four inches to the right of the median line a mass about the size of an orange is felt just below the border of the liver; its position, shape, and resistance make it correspond to an enlarged gall-bladder; other organs apparently healthy.

On the 23d of July a longitudinal incision was made about six inches in length through the external border of the rectus abdominis; peritoneum opened; gall-bladder not adherent to the abdominal wall; its color light yellowish-green. By aspiration about one and a half pints of a rather thin purulent fluid were extracted; toward the end of the aspiration the same showed a more free admixture of pus; microscopically pus-cells and cholesterin crystals were found. The amount of fluid withdrawn was much more than at first seemed probable from the size of the tumor; that proved, of course, a considerable dilatation of the gall-ducts beyond the gall-bladder itself. After turning the patient on her right side, and pulling the now flabby gall-bladder partly before the abdominal wall while interposed sponges secured closure of the peritoneal cavity, a free incision was made into the gall-bladder, from which more fluid escaped. Neither finger nor instruments, curved as well as straight, reached a stone, though the cavity was explored to the depth of about eight inches. After having enlarged the abdominal wound by an incision at right angles to the first, I passed my hand along the lower surface of the liver toward the duodenum, and there was able to distinguish by the touch a hard, round body. In order to get at it with ease, I had to separate the colon from its physiological and pathological adhesions to the lower surface of the liver, and then was able, by gentle pressure and manipulation, to push the stone back into the gall-bladder, whereupon it was easily extracted. Hemorrhage was insignificant on account of double ligature before cutting adhesions, which were most extensive at the lower surface of the gall-bladder. The stone was about the size of a small walnut, oval-shaped, smooth, without facets, consisting of pure cholesterin.

The patient was then again turned on her right side. The bladder was irrigated with a weak solution of boro-salicylic acid; the gall-bladder closed by triple rows of Lembert sutures, and, finally, the abdominal wound sewed up by deep peritoneal and superficial sutures; light dressing of iodoform gauze.

The operation lasted more than two hours, and the patient was in collapse immediately after it, but soon rallied, and in the afternoon had a moderately good pulse, with a temperature of 101°–102°.

The principal features of the following days until her death, which occurred on the morning of the 26th, three days after the operation, were these:

Vomiting, gradually increasing in severity, sometimes with pus; on the second day, with free admixture of blood; no bile. One large clot, about three inches long, looked as though it must have come from the bile-duct; in one place it consisted mostly of fibrin, and was somewhat constricted; the remainder being apparently a more recent coagulum. Microscopical examination showed much columnar epithelium caught in it. Urine became quite scanty, containing albumen and epithelial and hyaline-granular casts, which were not present before. Abdominal pain and tympanites moderate; temperature gradually increasing to 104° on the third day. The fact that the vomit contained no admixture of bile, and the stools, which were occasionally passed, had retained their grayish color, led me to assume that in some way the flow of bile into the gut might be interfered with. I therefore, on the 25th of July, opened the abdominal wound, and finding safe adhesions between the gall-bladder and parietal peritoneum, opened the former, giving exit to a considerable quantity of green, healthy-looking bile. The bad symptoms continued in spite of this, and on the morning of the 26th the patient died, having a temperature of 108.2°; quantity of urine during the last twenty-four hours was twelve ounces, about the average of the days before.

The autopsy made on the same day was only conceded in reference to the abdomen. The liver had, within those few days after the operation, returned to almost its normal size. The whole system of gall-ducts was much distended; the ductus choledochus, which might have admitted a finger, ended with a round lacuna within the walls of the duodenum, where apparently the impacted gall-stone had been located. Several small openings led from here into the gut, which all along the small intestine was filled with a brownish, chocolate-colored material, similar to that vomited during life. The essential points of the microscopical examination, which I owe to the kindness of Dr. Porter, pathologist to the Presbyterian Hospital, were these: "Hepatic cells in a state of granular and fatty metamorphosis, which had completely destroyed their identity. There were also marked biliary pigmentation and slight interstitial thickening. The sections of the liver presented the appearance common to acute yellow atrophy of the liver. Sections of the kidney showed the most advanced stage of acute parenchymatous metamorphosis. The epithelium was swollen and the protoplasm destroyed and replaced by granular particles and fat droplets, the latter predominating. The points of discoloration upon the intestine appeared to be ecchymotic.

"The cause of death in this case may be ascribed to the toxæmic condition produced by the advanced

metamorphotic destruction of the hepatic and renal cells."

I have been, and am still of the opinion, that an acute septic peritonitis might have caused all the above changes, though nothing more of an exudation could be discovered on the surface of the peritoneum than is necessary to account for an adhesive inflammation on the separated parts. There have been, however, cases of a peritonitis septica under observation where the macroscopic changes were either absent or quite insignificant. From a practical point of view the following considerations may be allowed:

1. Would it eventually be preferable, as recommended and done by others in cases of cholecystotomy, first to establish a fistula of the gall-bladder, operating *en deux temps* in order to be as secure as possible against infection of the peritoneum? If then after free opening of the gall-bladder the stone cannot be reached, apparently on account of the curved course of the bile ducts, would it eventually be the safest plan to leave it alone and trust to its final spontaneous perforation into the intestine? In our case, as shown by autopsy, this natural process was already inaugurated. In spite of all possible care and precaution it is very difficult, during an operation lasting several hours, to prevent the entrance of infectious material. I need not to say that antiseptic measures were used to my best knowledge in my case.

2. One might, before opening the gall-bladder, search for the stone by palpating along the cystic and common duct. If found near the duodenum, one could try to dislodge it with the intention of pushing it back into the gall-bladder. If one succeeds in doing so, the operative procedure first mentioned might follow.

3. To excise a gall-stone impacted in the common duct *in situ*, seems to me, on account of the deep situation and inaccessibility of the operating field, and the probable impossibility of preventing the contents of the duct from entering the peritoneum, a hopeless way of proceeding. Should it prove to be impossible to push the stone back or to extract it from above, an artificial communication between gall-bladder and intestine, according to Winiwarter's idea, might come into consideration.

If I were to operate a second time under the same conditions as those above described, I should first puncture the gall-bladder, and with a fine needle withdraw as much fluid as possible. I should then try to define the point of obstruction, and, in a case like mine, push the stone into the apex of the gall-bladder. Then I should close off the peritoneum by sutures around the partly protruding gall-bladder, and after that is achieved, open the latter, extract the stone, and not close the gall-bladder until the spontaneous entrance of bile into the gut had manifested itself.

Mr. President: All these questions can only be settled by experience. I purposely omit quoting those valuable contributions which have lately been offered relative to this subject. My case just recorded, through the location of the stone and the difficulty of getting at it, has some peculiarities of its own, and may form, in spite of its unfortunate end, a valuable addition to our present knowledge.

THE ABORTIVE TREATMENT OF BUBO.

BY PHILIP F. HARVEY,
ASSISTANT SURGEON U. S. A.

THE article entitled "Venereal Bubo," by D. T. Armstrong, Ph.D., M.D., etc., in THE MEDICAL NEWS of December 5, 1885, merely refers to the injection of dilute carbolic acid in the abortive treatment of that painful affection, but omits all mention of the method originally described by Dr. Morse K. Taylor, U. S. A., in *The American Journal of the Medical Sciences* for April, 1882. Dr. Taylor, in his paper, cites a number of cases in which his treatment ended in speedy and complete recovery, and for over three years I have treated all cases of bubo that have fallen under my charge in the manner he describes. Having seen no mention of his method in the medical journals, and as some inquiries among my surgical friends show that it is not as generally known and practised as it should be, I am induced to call renewed attention to it by briefly reciting some of my cases.

To save the trouble of referring to Dr. Taylor's original article, I will briefly recapitulate the successive steps of his method: The skin covering the bubo is first chilled with a spray of ether; the enlargement is then grasped between the thumb and finger of the left hand, held firmly, and the point of a hypodermic syringe thrust obliquely into the centre of the gland, to a depth equalling about two-thirds of the narrowest diameter of the tumor. The syringe being charged with an 8 gr. solution of carbolic acid to the ounce of distilled water, twenty to thirty minims are slowly injected. A bag of shot or sand, weighing about three pounds, is then placed on the bubo and held in place by tapes; although in favorable cases this may not be necessary. The stage of the disease at which the injection yields the best results is when the gland has attained a sufficient size to admit of the free infiltration of the injected fluid throughout its structure. At such time, one thorough injection is usually sufficient to effect a cure, and this without the patient foregoing his usual avocation. When the gland is smaller and harder, it may be necessary to repeat the injection once or oftener.

CASE I.—In the spring of 1882, Pvt. E., 11th Inf., was affected with double inguinal bubo arising from chancroid. He had been under the customary treatment for some weeks in hospital, and one gland had suppurated, but was not open; the other was large, but fluctuation could not be detected in it. The bubo containing pus was aspirated, and the cavity hyperdistended with carbolic acid solution. Aspiration failed to detect pus in the other bubo, and it was injected with ℥o minims of an 8 gr. to ℥j solution of carbolic acid. Marked improvement followed this treatment, and two more injections sufficed to effect a perfect cure.

CASE II.—S., colored, servant. Male. Applied to me in April, 1882, for treatment for a recent and hard inguinal bubo. It was injected in the manner described, and the patient was not seen again for three days. The bubo was then injected, and complete cure followed in a few days without further treatment. This man did not lose an hour from his work.

CASE III.—Pvt. M., 25th Inf. (colored). Affected with a bubo that had been treated by iodine and poulticing for one month. It was rather large, somewhat soft, and quite painful, and was evidently on the point of breaking down. The man was taken into hospital, put to bed, and treated by the abortive method. Three injections effected a perfect cure. Time under treatment, one week.

CASE IV.—Military convict. Treated while in guard house. Single inguinal bubo; cured by two injections.

CASE V.—Pvt. D., 17th Inf. (white), was taken into hospital July 6, 1882, with double inguinal bubo. The hospital steward having been instructed in the proper mode of making the injection, was directed to administer it in this case. The patient declined to allow the steward to "try experiments on him," insisting that poulticing was the best method of treatment. On the 7th inst., I injected the bubo in the left groin, which was larger than its fellow, and exquisitely painful, notwithstanding the patient's objections. On the following day this soldier requested me to inject the bubo on the right side. He said the pain was at once relieved in the left bubo, and had not returned. No other injections, however, were necessary in this case, and the patient was returned to duty in a few days afterward.

CASE VI.—D. S. G., recruit, 21st Inf. (white). Inguinal bubo; right side. Injected March 21, 1883; returned to duty March 22, 1883.

CASE VII.—Pvt. J., 25th Inf. (colored). Admitted to hospital in February, 1883, with large and inflamed chancroid; treated with iodoform. Multiple buboes ensued; they were injected on the 11th, 13th, and 16th. Result, prompt cure.

CASE VIII.—Pvt. S. P., 25th Inf., chancroid and right inguinal bubo; recent; no pus. Presented August 7, 1883. Injected bubo, using spray and compress. Kept patient in quarters. August 8th, no pain; swelling less; injected a second time. 10th. Bubo diminishing; again injected. On the 15th, the bubo having not disappeared, it was evident that walking about was keeping up irritation. He was therefore taken into hospital and kept at rest until the 25th, when he was fit for duty.

CASE IX.—Pvt. J. W., (white) a prisoner, having just arrived at the post, applied September 12, 1883, for treatment. In his left inguinal region there was a large swelling, soft and extremely painful to the touch, and of red, angry appearance. He was wearing a truss over it, as his physician had informed him that he was ruptured. His truss was removed and he was kept at rest until the following day, when his bubo was aspirated, about eight ounces of thick pus being withdrawn; and phenol solution being afterward injected. On the 17th he was admitted to hospital. On the 22d, the pus was again aspirated, and cavity injected to hyperdistention, the fluid being afterward withdrawn. 23d. Pus having again collected, the bubo was freely opened. The resulting abscess ran its course, although I believe it was decidedly shortened by the phenol injections. The man was returned to duty on the 12th of October.

CASE X.—Civilian, tailor; July 15, 1883. Chancroid, followed by bubo. Injected twice; no return after second injection.

The above cases were all treated at Fort Snelling, Minnesota. My notes and practice embrace other cases, but the above seem sufficient to establish the value of the method; in my opinion, it is the best mode of aborting or treating bubo that has yet been described. It may answer well in other forms of glandular inflammation, but as to that I have had no experience.

To insure a full measure of success, the following details should be borne in mind and carried out:

1. The time of election is just prior to suppuration.

2. Rest, a laxative, and reduced diet assist in the dispersion of the swelling, rest being the most important adjuvant.

3. The exact point at which the injected fluid should be delivered in the bubo, is a matter of importance; as also its manner of delivery. These conditions can be satisfied, however, by the exercise of ordinary intelligence and care.

1733 G ST., N. W., WASHINGTON, D. C.

MEDICAL PROGRESS.

PREPARATION AND PROPERTIES OF PEPTONE.—In *The Schweiz. Wochenschr.*, xxiii. p. 381, is given the following description of the properties and method of preparation of peptone: 5 kilogrammes of finely chopped lean beef are placed in a porcelain evaporating dish with 5 kilogrammes of water, 150 grammes C.P. concentrated hydrochloric acid and 20 grammes (Witte's) pepsin; allowed to stand at ordinary temperature for one day, stirring frequently; it is then heated in a water-bath, taking care not to heat the mixture to more than 70° C. for one day. The excess of acid is neutralized by sodium carbonate (requiring about 150–160 grammes). The resulting turbid solution is brought up to 10 kilogrammes, 5 kilogrammes of concentrated alcohol added and then put aside for one day to settle. The precipitate is collected on a strainer, expressed and the liquid filtered. After recovering the alcohol the solution is evaporated to extract consistency, poured on plates and dried. Peptone prepared thus is in brown pieces, quite brittle, yielding when pulverized a yellowish-brown powder, soluble in at least 2 parts of water. The yield is about 4–6 per cent. of the meat used. *Properties:* 1. Peptone should be soluble in 2 parts of water; the solution is not gelatinous, and becomes turbid on adding 5 volumes of absolute alcohol; the addition of more water makes a clear solution. 2. A 10 per cent. solution does not become turbid either at an ordinary or elevated temperature on adding nitric acid, acetic acid, ferrocyanide of potassium, or saturated solution of sodium sulphate. 3. Picric acid produces yellow—tannic acid, ash-gray flakes. 4. Sulphate of copper and caustic potash produce a violet coloration. 5. It should yield not more than 2 per cent. of ash when incinerated. 6. If to 20 drops of a 1 per cent. solution of peptone 5 drops of a 10 per cent. solution of calcium bichromate are added, no turbidity is produced; if the mixture becomes turbid, it proves the presence of at least 5 per cent. of glutinous matter. The solution of bichromate of calcium (Freire's test) is made by dissolving

5 grammes of crystallized chromic acid in 25 grammes of water, gradually adding 2 grammes of pure calcium carbonate; after effervescence the solution is diluted to 60 c.c. and filtered through glass-wool. Owing to the rapidity with which peptone is decomposed, it is not advisable to keep it in liquid form.—*Amer. Journal of Pharmacy*, Jan. 1886.

DANGERS OF NERVE-STRETCHING.—In an elaborate original investigation from Mierzejewski's laboratory, PAULINE TARNOWSKI records the results of an examination of the spinal cord in thirty-six rabbits, in which the sciatic nerve had been stretched in varying degrees. The amount of stretching was estimated from the weights employed—the maximum being eleven pounds. The examination of the cord was made at periods of from twenty-four hours to six months after the operation.

After slight stretching of the sciatic there was an evident hyperæmia of the posterior column of the cord on the same side. Intense stretching was followed by capillary hemorrhage in the posterior column, with secondary sclerosis and atrophy. After extreme traction upon the sciatic, capillary hemorrhage in the corresponding side of the posterior column could be traced to the cervical region.—*Centralb. f. klin. Med.*, December 5, 1885.

NEW METHOD OF PREPARING FEHLING'S SOLUTION.—M. SCHMIEDEBERG uses mannite in place of Rochelle salt in making Fehling's solution. He claims that the addition of mannite insures stability to the solution. His formula is as follows: Dissolve 34.632 grammes of crystallized copper sulphate in 200 c.c. of water; to this solution is added a solution of 15 grammes of very pure mannite in 100 c.c. of water and 480 c.c. of solution of caustic soda (1.145), and, lastly, sufficient water to make 1 litre.—*Amer. Journal of Pharmacy*, Jan. 1886.

ODORLESS IODOFORM.—OPPLER has accidentally discovered that coffee masks completely the odor of iodoform. The roasted coffee is finely pulverized and added to the iodoform in the proportion of from thirty to fifty per cent.

The following formulæ have been given:

Iodoform	2 parts.
Pulverized coffee	1 "

Mix.

Iodoform	3 parts.
Vaseline	30 "
Pulverized coffee	1 "

According to Gillette, one hundred parts of iodoform may be rendered inodorous by the addition of one part of sulphate of quinia, and three parts of powdered wood charcoal.—*Revue de Thérapeutique*, Dec. 15, 1885.

UNILATERAL ŒDEMA.—A case is reported by DR. BASSI in the *Rivista Inter. di Med. e Chir.*, in which an hysterical young woman presented the curious phenomenon of an unilateral œdema that appeared with each menstrual period. There was no other derangement of general health, although the patient is described as delicate. The œdema was absolutely limited to the right half of the body, but there was no alteration in the color, the temperature, or the sensitiveness of the two sides, although less irritation was necessary to produce reddening of the skin on the right side. Concurrently with this temporary œdema there were signs of congestion of the lung on the same side, and the right ovary was painful. Complete anæsthesia of both corneæ and of the fauces was observed.—*Lancet*, Jan. 2, 1886.

DIURETIC MIXTURE FOR GOUT.—J. MORTIMER GRANVILLE, in his recent work on *Gout in its Clinical Aspects*, discards the usual diuretics as irritant, the indication being to "flush," not to stimulate the kidney. For this purpose the most available drugs are ammonium chloride and potassium chlorate. At the same time the decomposition of the sodium urate in the blood may be attempted, and to this end Dr. Granville considers iodine best adapted, exhibiting it with the salts and glycerine, as exemplified in the following formula:

℞—Ammonii chlor.	ʒiv.
Potassi chloratis	ʒij.
Tinct. iod.	♏cxx.
Glycerini	ʒiss.
Aquæ	ad ʒxvj.—Misce.

F. Mistura, cujus sumantur cochlearia duo magna quartâ quâque horâ et aquâ.

MUSCLE-GRAFTING.—The following conclusions are presented by DR. SALVIA (*Gazetta degli Ospitali*) as the results of his experiments in muscle-grafting.

1. It is always possible to transplant a portion of muscular tissue from one animal to another, differences of species having no effect upon the definitive result of the operation.

2. In order that the muscle added shall become united by first intention, it is necessary that the portion grafted shall exactly fit the wound into which it is introduced, and that the strictest antiseptic precautions be instituted.

3. At the point of union there is developed new muscular tissue with no trace of a cicatrix.

4. The added muscular elements gradually lose their special anatomical peculiarities and so thoroughly acquire those of the animal upon which they are grafted, that after a certain time microscopical examination reveals no point of difference.

5. Muscular function is resumed in a relatively short space of time.—*Revue Médicale*, Nov. 14, 1885.

PULSE VARIATIONS IN HEALTH.—BLEULER and LEHMANN record a series of experiments made upon themselves with a view to determining the results of certain every-day procedures upon the pulse.

The experiments were made in bed, during the morning hours, the pulse was counted for a full minute upon each occasion, and all the accessary conditions carefully noted. The results were as follows:

Drinking warm water or tea increased, while cold drinks diminished the pulse frequency. Application of heat to the skin, effected by additions to the bedclothing increased pulse frequency, which was decreased by partial exposure of the body surface. The difference amounted to from ten to fourteen beats per minute, and the effect endured for from thirty to forty minutes.

During twenty-four hours of fasting, quietly spent in bed, no changes in the pulse frequency were noticed.

Mental activity—in the form of light reading—as a

rule slightly reduced the cardiac activity, and in no case increased it.

Various gastro-enteric sensations—those of heat or pressure in the stomach, nausea, the sensation of slight intestinal tension, especially in the rectum—all increased the frequency of the pulse.

A similar effect was produced by the ingestion of sodium chloride or sodium sulphate, and explained by the fact that these salts produce the sensations in question.—*Centralb. f. klin. Med.*, December 5, 1885.

ETIOLOGY OF GOUT.—In a work which has just appeared on *Gout and its Relations to Diseases of the Liver and Kidneys*, DR. ROOSE thus summarizes the causation of the attacks of gout:

1. Uric acid, in the form of sodium urate, is the *materies morbi* of gout.

2. The deposit of sodium urate in the joints is the cause of the gouty inflammation.

3. This substance is produced in excess, as a result of the imperfect transformation of albuminous substances.

4. This imperfect transformation is for the most part due to functional disorder of the liver, or to excessive supply of nutritive materials, or, as often happens, to a combination of these causes.

5. So long as the excess of uric acid is eliminated by the kidneys, decided attacks of gout may be absent; but the symptoms above described as pertaining to the uric acid diathesis are liable to be present.

6. The kidneys are apt to become *secondarily* affected, owing to the irritation set up by excess of uric acid and other products of defective metamorphosis, and by deposits of urates. Primary disorder of the kidney is not a necessary factor in the production of gout.

7. In the majority of cases of chronic gout, increased production of uric acid is associated with defective elimination by the kidneys.

8. The symptoms of nervous disorder in gout are due to the action of the *materia peccans* on the nerve-centres.

THE EFFECTS OF TOBACCO.—In a recent original investigation (*The Physiological and Pathological Effects of the Use of Tobacco.* Phila., P. Blakiston, Son & Co., 1885), which has received the Fiske Fund Prize, DR. HOBART AMORY HARE, of Philadelphia, thus summarizes the results of his experiments upon the effects of tobacco.

1. Tobacco smoking does not decrease the urine eliminated, but rather increases it.

2. Tobacco does retard tissue waste.

3. Tobacco and its alkaloid cause convulsions in the primary stage of the poisoning, by depressing the reflex inhibitory centres in the cord.

4. It causes the palsy of the second stage, by paralyzing (*a*) the motor nerve trunks, (*b*) the motor tract of the spinal cord.

5. That the sensory nerves are not affected by the drug.

6. That nicotine contracts the pupil, by stimulating the oculo-motor and paralyzing the sympathetic, this action being peripheral.

7. That nicotine primarily lowers the blood pressure and pulse-rate; (*a*) secondarily increases pressure and rate; (*b*) thirdly decreases pressure.

8. That the primary lowering of pressure and rate is due to pneumogastric stimulation, associated with vaso-motor dilatation.

9. That the secondary stage is due to vaso-motor constriction and pneumogastric palsy.

10. That the third stage is due to vaso-motor dilatation returning.

11. That death in poisoning from this drug is due to failure of respiration, the action of the drug being centric.

12. That the blood corpuscles are broken up and crenated by the action of the poison.

13. That in death from nicotine poisoning the blood shows changes in *spectra*.

14. That death can be brought about by the cutaneous absorption of nicotine.

15. That tobacco in moderate amounts increases intestinal peristalsis, and in poisonous doses produces tetanoid intestinal spasm.

16. That the liver seems to destroy the poison, although this destruction is participated in by any set of capillaries in other parts of the body.

17. That tobacco smoking increases pulse-rate and decreases arterial pressure.

TENDON-GRAFTING. — MM. FARGIN and ASSAKY have reported before the Société de Biologie the results of their experiments on the grafting of tendons.

Under rigid antiseptic precautions, the transplantation of one inch of tendon has succeeded, not only in animals of the same species, but between different species.

The animals operated upon were dogs, sheep, rabbits, and birds.—*L'Union Médicale*, Dec. 17, 1885.

POTASSIUM SALICYLATE IN ACUTE RHEUMATISM.—DR. E. L. MILLER, of Eaton, N. Y., recommends the use of the potassium instead of the sodium salt of salicylic acid in acute rheumatism. He claims that the potassium favors the elimination of the worn-out albuminoid products which fall short of being converted into urea in the body; he has used this salt when the stomach refused to tolerate the sodium salt.—*Cincinnati Med. News*, Dec. 1885.

PILOCARPIN IN PUERPERAL CONVULSIONS.—DR. G. T. M'KEOUGH describes three cases of puerperal convulsions in which pilocarpin was exhibited, and draws the following conclusions in regard to the use of this drug:

"When the coma is profound, and has almost extinguished the action of the reflex centres, pilocarpin is a dangerous agent, on account of the impossibility to the patient of getting rid of the enormous quantity of bronchial secretion and saliva which floods the respiratory passages; but in those cases in which the physician is called before the patient has many convulsions—when the poison has not suppressed entirely the action of the reflex centres—when the patient is partially conscious, probably restless, and moaning, or when convulsions have not occurred, but seem imminent,—it is my conviction that we have in pilocarpin a most valuable adjunct in the treatment of this dreaded disease."—*Canadian Practitioner*, January, 1886.

ALCOHOLIC PARALYSIS.—In the current number of *Brain*, Prof. Dreschfeld has some interesting further ob-

servations on alcoholic paralysis. He has found that Magnus Huss, who is generally credited with having been the first to describe a paralytic form of chronic alcoholism, was preceded by thirty years by Dr. James Jackson in the United States. The latter wrote in 1822. Dr. Dreschfeld has clinically divided this affection into two groups—alcoholic ataxia and alcoholic paralysis—a classification followed by Lowenfeld, Moëli, Schulz, Krücke, and Strümpell. Alcoholic paralysis is a multiple peripheric neuritis.—*British Medical Journal*, Jan. 9, 1886.

PHYSIOLOGY OF THE SECOND AND THIRD STAGES OF LABOR.—SCHRÖDER, in the *Zeitschrift f. Geb. u. Gynec.*, xi. 2, presents the following conclusions regarding the processes involved in the second and third stages of labor:

During the stage of expulsion, the energetic action of the abdominal muscles is very essential, and, therefore, narcotics, since they calm excitement and diminish pain, are the best promoters of labor. Ergot and its preparations are, as a rule, contraindicated, since the muscles of the uterus contract of themselves sufficiently. During the third stage of labor it should be remembered that the mechanism through which the placenta becomes detached and is expelled from the uterus is a precise one, whilst the expulsion of the placenta from the vagina is not. The control of the uterus by means of the hand after the birth of the child is, therefore, unnecessary. The placenta will, normally, be soon expelled from the uterus through its contractions, without much hemorrhage. We know that this has happened when the uterus becomes smaller and its lower segment swells out over the symphysis. Then by simple manual pressure, above the placenta in the neighborhood of the contraction ring, we may easily, and without danger, completely expel the placenta. In typical cases, the rational and best method of conducting the third stage of labor is to wait until the placenta has been expelled from the uterus, and then resort to manual pressure. When the placenta is expelled after the manner described by Duncan, then the hemorrhage is apt to be greater, and manual pressure not so effective or simple, since the upper border of the placenta often remains for a time attached at the fundus.—*Am. Journal of Obstetrics*, January, 1886.

THE ADMINISTRATION OF THE HYPOPHOSPHITES IN POWDER.—DR. E. C. MORGAN, of Washington, states that practitioners frequently encounter patients in whom the ordinary saccharine preparations of the hypophosphites cause serious gastric disturbance, loss of appetite, and interference with nutrition. Individuals suffering from diseases in which hypophosphites are indicated often possess an intolerant digestive apparatus, thus rendering the administration of syrups questionable and occasionally impossible. The writer has, for these reasons, long been accustomed to prescribe sodium, potassium, calcium, iron, and other hypophosphites, in the form of powder, taken dry or stirred in wine, whiskey, brandy, milk, or water. Some of my professional colleagues have doubtless employed these salts in powder and it is for the purpose of directing more general attention to the subject that I write this article.

The therapeutic advantages resulting from the combination of pepsin and various salts of the hypophosphites are apparent, my attention being first called to the fact by Mr. J. S. Tyree, of our city.

The formula can be varied to meet special indications; but a most serviceable combination is as follows (pulv. hypophosphitum cum pepsino (Morgan)):

℞.—Calcii hypophosphitis . . 5 parts.
Sodii hypophosphitis . . . 5 "
Ferri hypophosphitis . . . 2 "
Mangani hypophosphitis . . 1 "
Pepsini (Boudault's neutral) . 8 "
Sacchari lactis . . . . 4 " —M.

This powder should be finely ground and kept, as well as dispensed, in glass. It can be prescribed in bulk, one-half teaspoonful representing about fifteen grains, the adult dose. The preparation is to be taken three times daily, after eating, and preferably in milk.

Lactopeptine, ingluvin, or scale pepsin, may of course be substituted for the neutral pepsin of Boudault, which latter, after repeated trials, I prefer.—*Maryland Med. Journal*, January 16, 1886.

AMMONIA IN ANTHRAX AND CARBUNCLE.—Dr. LEONIDAS AVENDANO, in a paper read before the Union Fernandina, a Lima medical society, testified to the great value of ammonia in anthrax and "carbunculous diseases." Indeed, he expressed his opinion that it "is a specific, and that it should be the only drug used." In cases of malignant pustule, he advises that after an incision has been made, some drops of the officinal solution of ammonia should be introduced into the wound with the hope of destroying the bacilli there, and of the ammonia finding its way into the circulation before the bacillus does, and thus converting the blood into a fluid in which the parasite cannot multiply. In addition, some ammoniacal salt, such as the acetate, should be given internally, and on the slightest suspicion of general infection resort should at once be had to intravenous injections of ammonia, in doses of ten drops of the officinal solution diluted with an equal quantity of distilled water. In cases of malignant œdema and carbunculous fever, too, "the microbe should be attacked directly in the blood, ammonia being injected into the circulation." The author detailed several cases in which immediate improvement and ultimate recovery had taken place in patients apparently moribund by the use of repeated ammoniacal intravenous injections. He concluded by claiming for Peruvian medicine the honor of having suggested and proved the value of intravenous medication in no less than four different diseases: Dr. Leno Alarco having first injected chloral in tetanus, and ammonia in septicæmia or purulent infection; Drs. Armando Vélez and F. P. del Barco having employed capsicum injections into the veins of yellow fever patients; and Dr. Néstor Corpancho and himself having originated the treatment above described in carbuncle. —*Lancet*, Jan. 9, 1886.

SUPPOSITORY FOR HEMORRHOIDS.—MARTIN recommends the following formula for use in cases of hemorrhoids:

Antipyrin . . . . . 38 grains.
Cocoa butter . . . . . 150 grains.

For five suppositories: two or three every twenty-four hours.—*Revue de Thérapeutique*, Jan. 1, 1886.

THE MEDICAL NEWS.

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SATURDAY, JANUARY 23, 1886.

PASTEUR'S RESEARCHES IN HYDROPHOBIA.

DEEP interest has everywhere been excited by the announcement of the results which Pasteur has recently obtained in reference to the prevention of hydrophobia by inoculation, and in the absence of full and authentic statements of the methods employed and results obtained, the press has been teeming with crude and unreliable accounts and criticisms, favorable and unfavorable, of the work done in Pasteur's laboratory.

We have the pleasure of laying before our readers, this week, a full and thoroughly accurate report of M. Pasteur's methods and results from the pen of Dr. F. S. Billings, of New York, who took four of the bitten Newark children to Paris to be inoculated for the prevention of rabies, and placed them under the care of Pasteur, and he has thus enjoyed exceptional opportunities for studying his methods and for obtaining full information concerning his researches on this subject.

Fortunately for the accurate estimation of the efficacy of Pasteur's inoculations, two children who were bitten by the same dog were not taken to Paris, and, with several dogs which were bitten at the same time, are also now under close observation, and constitute a valuable control experiment, the final result of which will be watched with eager interest.

The practical conclusions to which Pasteur's researches so far tend, are that dogs should be subjected to inoculation for rabies and thus rendered harmless, and that those which are not rendered rabies-proof should be put beyond the reach of doing harm.

EMMET'S OPERATION FOR VAGINAL PROLAPSE.

A GRACEFUL compliment was paid to one of the most eminent of the alumni of Jefferson College by an invitation to give a clinical lecture in the College Hospital. Nearly two score years have passed since Dr. Emmet entered Jefferson College as a student; since graduation he has attained an eminence such as few in the profession reach, he has done a very large amount of original work in gynecology, and his success brings honor to the institution in which he received his professional training. He was greeted by the large class of Jefferson College, between five and six hundred, by some students from other colleges, and many members of the profession.

Our readers will find in this number of THE NEWS an authorized report of his lecture, which was a clear exposition of some of his original methods of treating certain lesions of the neck of the womb, of the vagina, and of the pelvic floor. We do not propose any extended or complete review of the lecture; still less shall we attempt any criticisms of his operations, but we may say a word or two as to some of his statements, or theories and reasoning, for while in hearty agreement with his practice, we cannot always accept his explanations.

Dr. Emmet states that he does not believe a woman can give birth to her first child without more or less laceration of the cervix. In opposition to this statement we might refer to a case under our own observation, where a woman five years after her first and only labor had a cervix and os precisely like those of a nullipara. Moreover, if one will turn to page 103 of Tarnier's work, he will find that this distinguished obstetrican states that in two or three instances of women who had each borne one child, he found the os uteri presenting the same characters as in the nullipara, and that he has even met with a subject in her third pregnancy who presented to the touch a neck identical with that of a primipara. Evidently there was no laceration of the cervix in any of these cases.

Dr. Emmet regards his operation for the restoration of the pelvic floor as reuniting torn fascia, and that narrowing the vagina, in cystocele or in rectocele, is a misnomer. First, as to the last statement: We cannot see that if A, B, and C represent three parallel spaces, and we bring A and C together, crowding B completely out, there has not been a space eliminated. Now this is just what is done when two parts upon the sides of the anterior wall of the vagina are brought together over the median line, and there made to unite, as in Emmet's operation for cystocele. To say that the vagina after the operation is no smaller than it was before the birth of a child, is no explanation, for the vagina during labor is immensely enlarged, and probably has never returned to its original condition, and so the surgeon takes away redundant tissue that nature has failed to re-

move. Moreover, after giving birth to a child, a woman, no matter if labor and lying-in be perfectly normal, never has a vagina as small as she had before. If a vagina can be enlarged, why cannot it be lessened? If it cannot be expanded, why cannot it be contracted, or narrowed? Referring again to the letters just used, A and C were separated from each other, but they are now united, and we naturally ask where is the interval that separated them? We are answered that only "redundant tissue" was removed, just as a woman takes "a plait in her dress if it was too large for her." Supposing the dress to be a hollow cylinder, its diameter is surely lessened when the pleat is taken, and so, to our thinking, the vagina is certainly narrowed when two distant surfaces upon one of its walls are brought and kept together.

Now as to whether Emmet's operation—he has originated so many that the one referred to must be specified—designed to restore the pelvic floor does this by uniting torn fascia, we think admits of doubt. We have been struck with the force of some of Budin's remarks in reference to this point in his paper upon physiological and pathological contraction of the levator ani in the female, published in 1881. He states that the comparison of a purse closed with a cord is more easily comprehended with retractile and contractile muscular fibres than with the fibrous tissue of fascia. "One understands more readily, also, the great distention the bands which encircle the vagina can undergo at the time of labor without being ruptured, if their resistance is due not to fascial tissue, but to muscular fibres." In one word, according to Budin, it is the muscular fibres of the anal levator which are torn in some cases of labor, and not fascia. Hence the operation of Emmet restores the former to their integrity. Now that the anal levator has a most important influence in physiological narrowing of the vagina is a demonstrated fact, and hence, if its fibres are torn, the vagina is necessarily enlarged; while, if restored, we must believe that the vagina is narrowed by the operation.

One word as to the restoration of posterior displacements of the uterus, so often observed after an operation for a tear in the neck of the womb. The explanation is found chiefly, we believe, either in some inflammation of the utero-sacral ligaments, causing permanent contraction, or, remembering the name *retractores*, given to these ligaments, we believe, by Luschka, and that they contain a notable amount of muscular tissue, and too, as the researches of Bayer (*Morphologie der Gebarmutter*) show that they contribute largely to the formation of the muscular tissue of the neck and of the lower part of the body of the uterus, in the operation uniting torn muscular structure, and thus restoring to these retractors their original power.

THE TREATMENT OF PROFUSE HÆMOPTYSIS.

PROFUSE hæmoptysis is most frequently met with in chronic phthisis and in erosion of the trachea or bronchi by aneurisms. The initial hemorrhages of phthisis are rarely profuse, and in the stage of ulceration and softening we do not often meet with very free bleeding. Fatal hemorrhages in phthisis usually result from the bursting of small aneurisms of the pulmonary arteries in the walls of cavities, or from the erosion of mural or trabecular vessels which have not undergone the obliterative arteritis which usually precedes their destruction. Where a patient has died suddenly after a free hemorrhage, one or other of these conditions is almost invariably found. In most of the cases suffocation, rather than actual loss of blood, is the immediate cause of death.

A paper of DR. SAMUEL WEST, on the treatment of profuse hæmoptysis, recently read at the Medical Society of London, led to an interesting debate, in which the principles which should guide the practitioner in this serious emergency were fully discussed. Rest and quiet were naturally insisted upon, and the beneficial effect of opium to allay restlessness and excitement was referred to by several speakers. By some it was also regarded as hæmostatic. As in the majority of these cases a vessel is divided or open, not much benefit can be expected from drugs. Dr. West held that the belief as to the use of vascular constringents in pulmonary hemorrhage is based upon an incorrect theory of its pathology. Ergot, he thought, of doubtful value, as it constricts vessels smaller than those from which the bleeding comes. T. H. Green, and the President, Dr. Ord, thought that ergot is of benefit in reducing the blood-pressure. Oil of turpentine was the only other drug which seemed to be in favor with the speakers.

In these cases we have to deal with conditions precisely similar to those which exist in other parts of the body when a vessel is eroded or an aneurism is bleeding. The hemorrhage, if not immediately fatal, is checked by the contraction of the vessel and the clotting of the blood, which are greatly aided by the reduction in blood-pressure caused by the bleeding. Here we have a clue to the rational method of treating such cases. Unfortunately, the arteries from which the bleeding takes place are usually too far diseased to contract much, and we know of no drug which has the power of increasing the coagulability of the blood; but we can influence powerfully the blood-pressure, and so aid the clotting. We have known a bold venesection to check obstinate hæmoptysis in a plethoric man, but this measure is rarely to be practised in the cases under consideration. Other means must be resorted to, of which purgation is the most efficient, by dilating the abdominal vessels, and lowering the vascular tension in other parts. The same effect may be produced

in the vessels of the skin by amyl nitrite, the use of which was suggested by several speakers. Cardiac depressants, such as antimony and ipecacuanha, also influence the blood-pressure, and are thought valuable by some physicians. But, after all, the bleeding is probably the most efficient aid in reducing the blood-pressure and favoring coagulation at the orifice, and we may truly say, in some cases, though it sounds paradoxical, that if hemorrhage does not kill by suffocation, it may save.

The not infrequent recovery from sharp hæmoptysis in cases with pulmonary cavities, teaches us that the bleeding is often controllable even when a vessel or an aneurism of considerable size has given way. Rest, quiet, and a restricted diet, with free purgation and opium to allay the restlessness and cough, are measures the usefulness of which we can heartily endorse; but in these profuse hemorrhages we place little reliance on ergot, turpentine, or other hæmostatics.

THE INTERNAL USE OF VASELINE.

ATTENTION has been attracted to the effects of vaseline and allied hydrocarbons, when taken internally, by the recent decision of the Minister of Commerce in France that the introduction of these bodies into articles of food constitutes a fraud, and as such is indictable. The wisdom of this decision is made evident in the report of the French Consulting Committee on Public Hygiene on "The prohibition of the use of vaseline, petroleine, neutraline, and every other kind of similar products in the fabrication of cakes and of alimentary substances in general.

"Vaseline, not being liable to turn rancid, is fraught with serious danger to the consumer, who, unwarned by the smell of the age of the cake, runs the risk of purchasing pastry, the eggs used in which have already become tainted, and he only becomes aware of it when the pastry is brought under the control of the gustatory organs."

Apart from this, however, the question of the harmfulness of vaseline and its congeners is dependent upon two other considerations: first, whether it is intrinsically poisonous; and second, whether it is in any degree digestible.

In the first regard the evidence is negative. The lighter petroleums are notoriously toxic, but the heavy, residual "mineral oils"—in reality, soft paraffins—are entirely non-irritant even when applied to raw surfaces, and there is no proof that they are absorbed under these conditions. DUBOIS, of Paris, in the *Gazette Hebdomadaire*, of Nov. 27, 1885, has shown that in animals the ingestion of large quantities of vaseline is followed by no toxic symptoms whatever.

The question of the digestibility of vaseline is most important in regard to its introduction as a component in cooked articles of food. If it be digestible, as intimated by Dubois, then its presence is not likely to render such articles more irritant than similar foods containing butter or lard. If, on the contrary, it be indigestible, it will, when present in cooked foods as a substitute for the organic fats, form around the particles of such food a thin but absolutely resistant pellicle, with the result of rendering the food itself indigestible, and therefore, to a greater or less degree, irritant in its entire transit through the digestive tract.

The first study of this phase of the subject was made apparently by RANDOLPH, of Philadelphia, whose paper appeared in the *Proceedings of the Academy of Natural Sciences of Philadelphia*, in 1884, and who, during eight days, took daily one-half ounce of commercial vaseline. Digestion was in nowise altered, and no appreciable results ensued. Thereupon, to each of two healthy adults was given, in the course of forty-eight hours, one ounce of vaseline, and their feces for three days from the beginning of the experiment were collected, dried, and extracted with petroleum ether. With a slight allowance for incompleteness in extraction, the vaseline ingested was, in each case, recovered in its totality, showing that it had passed through the intestine unabsorbed and unchanged. The same writer, in a later communication in the *Therapeutic Gazette*, of Nov. 1885, states that he has utilized his previous observation by administering vaseline to a number of persons suffering from diarrhœas of irritation, with very satisfactory results. The drug in question, in view of its blandness and incapacity for absorption, probably acted as a soothing application to the irritated mucous surface of the entire bowel. The earlier observations of Randolph receive confirmation in some recent statements by JUNKER, which appear in the *London Medical Record*, of Dec. 15, 1885, and there is a reasonable hope that the method here indicated may prove of service in the treatment of enteric inflammation.

OPERATION FOR IMPERFORATE HYMEN.

THE best method of operating in vaginal atresia, whether from imperforate hymen, or from other cause, is still unsettled, particularly as to the immediate or the gradual evacuation of accumulated menstrual fluid, and as to washing out the uterus if rapid evacuation be used. As remarked by Barnes, a few years ago in a discussion upon the subject before the London Obstetrical Society, fatal cases do occur now and then, no matter which plan may be pursued. It is probable, however, since antiseptics have taken so important a place in therapeutics, and are so generally used in the operation referred to, that its mortality will be lessened. Nevertheless danger may come from the antiseptic itself, as suggested by the history of a case recently reported to the *Société de Chirurgie* of

Paris, and which we find published in the *Journal d'Accouchements*, of December 30th.

A girl twenty years of age, had for a year suffered from severe abdominal pains; she had never menstruated, and emmenagogues were given her without effect. Upon abdominal examination a tumor was found reaching nearly an inch above the umbilicus; at the vulva a violet-colored tumor, formed by the thinned and distended hymen, was found. The patient was anaesthetized, and a small antero-posterior incision was made into the imperforate hymen, this incision was afterward increased in size, and a transverse one made; gentle compression through cotton wadding upon the abdomen was made, and the liquid evacuated in three-quarters of an hour. A tube was placed in the vagina, and a two thousandth solution of corrosive chloride used to wash out the vagina; these corrosive sublimate injections were continued until the fifth day, when a slight mercurial stomatitis being manifested, they were substituted by a solution of carbolic acid. The patient promptly recovered.

M. BERGER, the reporter, recognizes the dangers that may occur from rapid evacuation in certain cases of menstrual retention, but these cases can be known in advance by the fact that the distended tubes are discovered by abdominal examination, presenting fluctuating tumors outside of the median line.

The fact that this patient suffered from mercurial poisoning, though this was only slight, proves that the normal vaginal mucous membrane has greater power of absorption than has usually been attributed to it, and suggests the importance of using weaker solutions of the corrosive chloride, especially if intra-uterine as well as vaginal injections are made.

In the discussion of the subject by the Society, M. Lucas-Championnière condemned the use of carbolic acid solution injections, fearing absorption, because of the solution coming in contact with such an extensive surface, and preferred iodoform as an antiseptic. Gillette suggested that to prevent reunion of the lips of the incision, the latter should be made by the thermo-cautery. Of course, when this is done, immediate rather than gradual evacuation of the menstrual fluid necessarily occurs.

It is questionable whether anæsthesia is advisable, when, as in Berger's case, a tense, thin hymeneal membrane is to be incised or punctured, for the operation cannot be painful. Further, the history of the case, prior to the operation, suggests the folly of giving emmenagogues in a girl who had so long passed the age, and had the development of puberty, without first ascertaining whether there was not a failure in menstrual excretion, rather than of secretion, and the case, therefore, one in which such medicines could accomplish no good and might do much harm.

SOCIETY PROCEEDINGS.

NEW YORK NEUROLOGICAL SOCIETY.

Stated Meeting, January 4, 1886.

THE PRESIDENT, W. R. BIRDSALL, M.D., IN THE CHAIR.

A CASE OF SOFTENING OF THE CORPORA STRIATA, WITH SPECIMENS.

DR. J. C. SHAW related the case, that of a gentleman thirty-eight years of age, whom he saw for the first time in July, 1883, in consultation with Dr. Fuller. The patient was a large man, healthy in appearance. About a month previously he had an attack of hemiplegia on the right side, with very temporary loss of consciousness. When Dr. Shaw saw him the arm had entirely recovered, but there was still dragging of the leg. The tendon reflex of that side was exaggerated. There had been no premonitory symptoms except that for six months previous the patient had at times been exceedingly irritable. He denied having had headache, or syphilis, and claimed that if it were not for the paralysis of the leg he would be in perfect health. At this time Dr. Shaw was in some doubt regarding the nature of the lesion; the diagnosis seemed to lie between cerebral hemorrhage and a possible meningo-encephalitis. Against cerebral hemorrhage were his age and the dragging of the foot, the toe scraping the floor; and about the only point which favored meningo-encephalitis was mental irritability. The patient would take no medicine except cathartics.

Dr. Shaw saw him again in consultation in June, 1884. He had suddenly become completely hemiplegic upon the left side; was unconscious and had stertorous respiration; he passed his urine and feces in bed. Recovery gradually took place from the unconscious state, paralysis remained on the left side. Trophic troubles began in the joints of the paralyzed arm, accompanied with pain. Patient would cry when spoken to, and could give no history of himself. At this attack Dr. Shaw concluded that there was cerebral arteritis, probably syphilitic, and thrombosis. The patient accused his wife of infidelity, became very noisy at night, and was sent to an institution, where he died in October, 1884.

At the autopsy the viscera appeared to the naked eye to be normal. Nothing special was observed in the meninges. The ventricles were distended with fluid, and contained granulations. The corpora striata were softened, due to arteritis of both middle cerebral arteries and their branches. There was secondary degeneration in the cord on both sides, and the case seemed to illustrate the unequalness of the decussating fibres on the two sides, the degeneration on the right side in this instance being much more marked than on the left.

Dr. Shaw said the points of interest to him were, that both arteries should have been involved in the arteritis, causing softening of the corpora striata on both sides. That the patient saw dogs, etc., at night, which might have been a form of visual hallucination. Lastly, the accusation of his wife of unfaithfulness, a symptom which is frequently met in chronic alcoholics, but rarely in other conditions.

DR. E. C. SEGUIN asked if there was not probably involvement of at least one internal capsule.

DR. SHAW replied that there was, and that the case probably was not one of limited involvement of the corpora striata.

DR. SEGUIN thought a point of interest was the fact that granulations were found in the ventricles, for this is a rare pathological condition except in general paralysis. It is sometimes found in other conditions, as chronic mania.

DR. SHAW said that the granulations in this case were marked. He had seen granulation on the floor of the ventricles in a variety of conditions in the chronic insane and others. Sections of the cerebral cortex did not show the lesions characteristic of general paralysis. Dr. Seguin may have noticed that the patient was unusually satisfied with his condition, almost amounting to a delirium of satisfaction.

DR. GRACE PECKHAM then read a paper entitled

MIRROR WRITING AND OTHER PATHOLOGICAL CHIROGRAPHY OF NERVOUS ORIGIN.

The paper was introduced by a reference to the expression of individuality in handwriting. "As the members of the body are the servants of the brain and carry out its will in motion, as the vocal organs express its thoughts in sounds, so in the act of writing the hand embodies the subtleties of cerebral action; it is therefore in writing that we may find expressed deviations from normal nervous conditions, sometimes too slight to be indicated in any other way."

The physiology of the handwriting was next considered. Poore's views with regard to the muscles employed in the act of writing were quoted. At no time in the act of writing are fewer than two nerves engaged. The cerebral centres which preside over writing and the inter-commissural tracts are not fully made out. Charcot and others think there is a word motor representative centre situated at the base of the left third frontal convolution near Broca's centre of speech; auditory centres, employed when writing to dictation, in the temporal convolution; ocular centres, used when copying, in the parietal convolution.

We have a right-handed writing to be written with a centrifugal motion, in one direction, across the page from left to right, a writing which taxes the weak muscles of the hands, and which takes four or five times as long as is necessary for the representation of a word, as is shown by stenographers, who write as fast as a person can speak. In view of the increase of writer's cramp due to the extraordinary amount of writing to be done by bookkeepers, book-writers, teachers, and savants, it was suggested that a new method of writing should be intelligently discussed and one inaugurated which should be less elaborate, less taxing upon the small and weak muscles of the hand; a handwriting in which the letters can be formed as in phonography, disconnected, not requiring such precise and complicated notions to make them legible, and which can be written from above downward, from right to left or left to right, as one may choose.

Bianchi, in Italy, and Erlenmeyer, in Germany, were almost the only ones who entered into the study of the pathological signification of the handwriting. The subject has been considered in a disconnected way as a symptom of some diseases treated of in various textbooks. However remotely alluded to, there has been a general acknowledgement of the diagnostic value of the handwriting of disease.

The handwriting may become pathological through the alteration of any portion of the mechanism involved in its production. The paper included only a discussion of the changes produced by abnormal conditions of the nervous apparatus. Alterations in the handwriting may be mechanical or psychical, ataxic or tremulous, or a mingling of the two forms. The ataxic form is physiological when seen in childhood, before the power of coördination of muscles for the complex act has been acquired. The tremulous form is physiological in the handwriting of the aged, in which also is seen a tendency to retrograde into childish handwriting. The principal pathological handwritings are:

1. Handwriting resulting from trauma to the nerves. 2. Mirror writing and agraphia, caused by injury to the cerebral centres. 3. Handwriting of general paralysis, caused by alteration of cerebral tissue. 4. Handwriting as influenced by sclerosis, cerebral and spinal. 5. Handwriting as influenced by functional disturbance, including paralysis agitans and writers' cramp.

Samples of handwriting can be shown occasioned by the interference with the conducting nerve-fibres. Erlenmeyer exhibits a very curious specimen in which there was injury to the radial nerve. The tremulousness, or, rather, interruption of the lines, especially of the upper stroke, is very peculiar.

It is to be regretted that handwriting following injury to nerves has not been more studied, as much of interest from a physiological as well as a pathological point of view might be attained.

At present, considerable discussion has been awakened with reference to mirror writing. Ireland called especial attention to it by his paper on the "Brain," in 1883. Hughlings-Jackson and Buchwald first discovered mirror writing. From experimentation and investigation of the subject, the writer reached the following conclusions:

First, that mirror writing may indicate a change in the perception of the nerve-centres of the brain, what Erlenmeyer would call retrographia. The patient suffering from this would read the reversed writing certainly as easily as the normal. This would be a means of diagnosis between a cerebral mirror writing and one from mechanical causes alone. Second, that from careful examination of twenty-five cases on record, no indication has been found that the mirror writing occurred from other cause than a mechanical one—that is, the person writing with the left hand could do so more easily in a centrifugal than in a centripetal direction, the former association of ideas and muscular action making this so. Therefore, persons weakened by disease, or weak-minded children, in using the left hand, would write centrifugally—that is, produce mirror writing. The reason lies, therefore, not in a pathological but in a physiological condition.

The subject of agraphia has been much discussed of late with that of aphasia. It was first spoken of by Marce, in 1856. There are on record very few cases of agraphia uncomplicated with aphasia. In agraphia there may be word-blindness, word-deafness, or a volitional motor disturbance, resulting in inability to write

The investigators of the subject have entered deeply into the discussion of the seat of the writing-centre in the left hemisphere. The unilateral theory is not so fully endorsed as hitherto. The diagnostic value of the handwriting in agraphia resides in the evidence it gives of the extent of damage done to the brain tissue in these cases of hemiplegia, and also an indication of the location of the trouble.

In the early stages, the handwriting of the insane indicates little the invasion of the disease; but when there are bad spelling and omission of words, it should awaken suspicion. The progress of the malady is marked by changes in the handwriting. It is the reverse of that of childhood. In the first stage there is a diminution of the handwriting, a running of the words together, and either not finishing the final letter, or terminating it in an irregular outline, or dropping it entirely. The letters are shaky, irregular, and there is a plentiful besprinkling of capitals. Later the letters are coarse, irregular, in uneven lines, and finally there are scattered pot-hooks and ataxic marks, like those of a child first learning to write.

The handwriting of multiple cerebro-spinal sclerosis is chacterized by tremulousness.

In locomotor ataxia involving the upper extremities, the changes are shown early when writing with the eyes closed, especially in round letters, and *l* and *e*. They are formed by straight lines more or less extended, instead of curves. When the disease is further advanced, the writing is ataxic when the eyes are open, though not so much so as when they are closed. Later there is inability to write a single word.

The functional disturbances of the handwriting include the writing of paralysis agitans and writer's cramp, as also that of chorea and hysteria. In the two latter there is nothing of special diagnostic value. The writing of paralysis agitans shows the tremor in the early stages. Even when the handwriting looks normal to the naked eye, a glass will reveal the peculiar character given it by trembling. In writer's cramp the writing shows many forms of disturbance, and were there no history, it would be difficult to say whether we were dealing with some of the various forms of tremor, paralysis agitans, progressive muscular atrophy, etc. A study of the handwriting will, however, reveal the nature of the disturbance, whether it is of the spastic, treumulous, or paralytic form.

DR. E. C. SEGUIN said that in his practice he has frequently derived considerable help in diagnosis from the patient's handwriting. He thought the medico-legal aspect of pathological handwriting might, after further study, assume considerable importance. It would be of value to be able to say in a court of law whether a given specimen of handwriting was that of an aged imbecile, a general paralytic, or an intoxicated person.

DR. LESZYNSKY thought we could hardly say from the specimens of handwriting of paralysis agitans presented that they were characteristic of that disease. He had a number of specimens of handwriting of chronic alcoholism, which were almost exactly similar. Dr. Peckham had not mentioned the handwriting of epileptics, nor had he read of anything characteristic about it, but he could recall two epileptics whose handwriting appeared to be rather unique. In some epileptics there is the condition known as the echo sign, being a repetition of words during conversation. He had seen the same thing manifested in the writing in two cases. He has recently had a case of telegrapher's cramp, in which the hand movements were markedly ataxic, being without any control.

DR. M. PUTNAM JACOBI has been struck by the great varieties and peculiarities in the handwriting of persons in usual health; some of the peculiarities supposed to be more or less characteristic of the handwriting of diseased states were also observable in the handwriting of certain persons in a state of health. For instance, Dr. Jacobi is unable, without giving special attention, to write in a direct line across the page; the line always sloped downward. The question has suggested itself to her whether, in the case of the insane, the tendency, especially after writing for a while, to slope the lines downward and to use capitals profusely, is not due to mental fatigue; and the use of capitals may be due to a desire to emphasize an idea which a fatigued and feeble brain is incapable of correctly expressing.

DR. DANA said he did not think it would be practical for everybody to learn short-hand instead of other writing, but he thought some modification of short-hand might be adopted. There would result perhaps greater facility of expression if present methods of writing were simplified, but there would no doubt be an increased amount of writing, which, perhaps, would be a greater misfortune.

Regarding mirror writing, he thought it is physiological. When we educate the centres to write with the right hand, we at the same time educate to some extent the automatic centres for left hand or mirror writing. If a person put a pencil in either hand and attempt to write his own name, he will find that the left hand will write mirror writing, while from the right hand the usual writing will result. If then by hemorrhage the centre for right handwriting be destroyed, it is perfectly natural for the centre controlling left handwriting, if not corrected by the will, to produce mirror writing.

DR. A. B. JUDSON presented a specimen of mirror writing by a negro girl, eleven years of age, produced ten years ago. She had slight right hemiplegia, but was able to perform household work with the right hand, and she could also write somewhat with it. The writing with the left hand was a fine specimen of mirror writing.

DR. PECKHAM closed the discussion, and said that the echo sign, the repetition of written words, was well shown in the third specimen presented, that of a woman who had right hemiplegia and amnesic agraphia. Regarding the use of capitals and the tendency to underscore by the insane, she thought they were in harmony with their exaggerated ideas.

MONTREAL MEDICO-CHIRURGICAL SOCIETY.

Stated Meeting, January 5, 1886.

THE PRESIDENT, T. G. RODDICK, M.D., IN THE CHAIR.

RAPIDLY GROWING OVARIAN TUMOR.

DR. TRENHOLME showed a large semi-solid ovarian tumor which he had removed from a woman some days before. The patient, when she consulted him, was unaware that she had a tumor, but consulted him for severe abdominal pain, which had lasted some three weeks. She had only noticed a swelling for the last six weeks.

Latterly, the tumor had grown very rapidly. It was removed without great difficulty, though the adhesions were numerous and the operation was complicated and prolonged by the bursting of a cyst. The pedicle was broad, and, as is his custom, he ligated it in sections with shoemakers' thread. The patient's temperature rose to 101° the day after operation, but soon fell to normal, and remained there.

DR. SHEPHERD mentioned that a short time ago he had operated in a case of ovarian tumor (in a childless married woman aged twenty-nine), with a history of only eight weeks' growth. The patient was seen by Dr. R. P. Howard a month before the operation, and at that time the tumor was of small size; it grew very rapidly, and in three weeks was quite large. At the time of operation, it was doubtful whether this rapid increase was not due to ascites. However, it proved to be a single ovarian cyst, with a solid base, containing twenty pints of thick fluid, and weighing some five pounds. The patient did well, and was able to return to her home in four weeks.

UNDEVELOPED BONES IN AN IDIOT.

DR. R. L. MACDONNELL showed the bones of the lower extremity of an idiot, which had been sent to the dissecting-room of McGill University from one of the institutions of the city. The individual was said to be twenty years of age, and had never spoken or walked. The bones, although of good length, were remarkably small, the femur not being thicker than an ordinary sized finger. The hip-joints were ankylosed in the flexed position, and there was contraction of the knees. The muscles of the lower extremities were strings of fibrous tissue with a little muscular tissue about them. The head, although somewhat microcephalic, was of good shape. In one femur there was a well-developed third trochanter.

HEMORRHAGE INTO THE PONS VAROLII.

DR. R. L. MACDONELL read the history of a case of hemorrhage into the pons Varolii. An old man, æt. sixty-two, was admitted into the Montreal General Hospital on July 31, 1885. He had been picked up by the police in the streets, and was in a semi-unconscious condition, unable to communicate anything whatever regarding his history. He was a tall, thin man, very anæmic, with wasted and flabby muscles. His expression was dull and listless, and, though he could utter words when spoken to, he was by no means rational. The pupils were equal, but the left was more sluggish than the right. There was slight paresis of the left side of the face, and the right side of the body was weaker than the left. There was increase of the superficial reflexes, but normal patellar reflex. The urine and feces passed in bed; he was always in a semi-comatose condition; pulse 90, and feeble. On the 3d of August, his breathing was stertorous, and the paresis of the left side of the face more marked, and coma more profound. Next day, the coma was complete, pupils contracted and unequal; large, moist râles heard at the bases of both lungs; toward evening, he died comatose.

The brain alone was examined after death, when a fresh clot was found in the pons Varolii, occupying the posterior or lower part, and situated rather more to the left than the right side. Dr. MacDonnell remarked that the central situation of the clot was shown by the equality of the paralysis on either side, and the greater weakness of the right side being accounted for by the position of the clot. It was a case of alternate hemiplegia, the left side of the face being paralyzed, though to a slight degree. This is characteristic of positive hemiplegia, especially when the lower half of the pons is injured, though usually the fifth and sixth nerves are also involved. There was nothing distinctive ih the condition of the pupils, which were not, as usually described, contracted, but merely sluggish in their reaction to light. In hemorrhage into the pons, one of two opposite conditions is usually observed: contraction of the pupils when the lesion is sudden and situated in the upper part of the pons, causing irritation of the nuclei of the third nerve; and dilatation from complete invasion and destruction of these nuclei.

DR. HENRY HOWARD asked if, at the post-mortem, the ruptured vessel had been found, as it was most important to know exactly the source of the hemorrhage.

DR. WILKINS related a case of very extensive hemorrhage into the pons, when the patient lived for eight days.

DR. STEWART asked if the whole of the left facial was affected or only the respiratory branches?

DR. MACDONNELL, in reply, stated that the ruptured vessel had not been found, and that the whole of the facial was paretic.

CEREBRAL SYPHILIS.

DR. GEORGE ROSS reported a case of supposed cerebral syphilis which had occurred in his wards in the General Hospital since his paper on that subject was read before the Society. The patient died a few days after admission, and a post-mortem was obtained. He came into hospital complaining of very severe pain in the head and vomiting. In a day or two he was quite maniacal, and then gradually became comatose. He died comatose five days after admission. At the post-mortem, Dr. Wyatt Johnson found at the base of the brain a single small flake of recent lymph, lying on and attached to the inferior surface of the facial nerve in the right side, near its origin. On slitting up the vessels at the base, this exudation was seen to correspond to a small lateral branch of the basilar artery, where it crossed the nerve. The thrombus extended in this vessel as far as its origin from the basilar, at which point a small roughened, reddish patch existed in the intima, and its wall was thickened, but no thrombus present. The right posterior cerebral artery presented a thickened wall and narrow lumen, and was thrombosed in its whole course; other cerebral vessels normal. On dissecting the brain, no local degenerative changes were recognized anywhere. Dr. Ross remarked that it was singular that such apparently simple lesions should produce such grave symptoms. He had expected to find much more marked pathological changes in the brain.

TREATMENT OF ERYSIPELAS IN INFANTS BY ZINC PAINT.

DR. A. D. BLACKADER read a short paper on the local treatment of infantile erysipelas. After giving an account of the treatment by white lead paint, which he had used to some extent, he said that it had occurred to him to use zinc paint instead in infants less than a year old, so as to avoid any risk of lead poisoning. He used

zinc white made into a paint with linseed oil, and found it a very soothing and cleanly application. He brought forward this method of treating erysipelas as being more easy of application, and more rapid in its results than the old treatment by lead lotion, cotton-wool, corn starch, etc. No bandages were applied to get disarranged or soiled. Whenever the parts are exposed, all the mother has to do is to apply the paint. In this way the application is more continuous, and the results are in consequence better. At the same time that he used local treatment he employed also constitutional treatment by iron and stimulants. He believed that erysipelas is a constitutional disease, and needs constitutional as well as local treatment. He reported a number of cases in very young infants, where the pain and irritation were immediately allayed by the application of the paint, and where the temperature soon fell.

POSTERIOR CERVICAL LACERATION OF OS UTERI.

DR. T. JOHNSON ALLOWAY reported a case, illustrated by diagrams, of severe posterior cervical laceration. The patient had suffered for years from violent hysterical attacks, and was never free from pain—in fact, she was a confirmed invalid. The parts were pared and brought together, with a most satisfactory result. The woman is now as well as ever, and able to attend to all her household duties. Dr. Alloway remarked that severe posterior laceration is a very rare affection; the slighter forms are not uncommon, and unite without operative interference.

SALIVARY CALCULUS.

DR. HINGSTON exhibited a large salivary calculus, which he had removed a few days ago from a lady who had been sent to him for supposed cancer of the tongue. There was great inflammatory swelling of the under part of the tongue.

BALTIMORE ACADEMY OF MEDICINE.

Stated Meeting, January 6, 1886.

DR. J. J. CHISOLM presented the following notes on

A PIECE OF IRIS LIVING IN THE VITREOUS CHAMBER.

G. T. S., aged fifty-nine, six months ago fell down a railroad embankment, receiving an injury to the right eye. When seen by his family physician six hours after the accident, he was suffering intensely. The lids had already become very much swollen. Upon carefully separating them, a wound was discovered at the upper edge of the cornea, from which a thick splinter of wood was protruding. The removal of this bit of wood was accompanied by a bloody aqueous discharge, leaving, however, so much blood in the anterior chamber as to conceal the amount of injury done to the eye contents.

The treatment pursued was rest, cold applications, and the internal administration of anodynes. As the swelling subsided and the blood was absorbed, it was found that sight had been so materially impaired as to leave only perception of light. In time, however, the vision slowly improved, till after three months large objects could be again recognized. In the mean time, the left good eye indicated some growing impairment of vision for distant objects, and the spectacles heretofore worn with comfort no longer permitted easy reading.

This growing defect of vision in the good eye was supposed to be a sign of sympathetic complications, and for this reason the patient was sent to me for surgical treatment.

I found the left good eye hyperopic, V. $\frac{15}{40}$; with a $+\frac{1}{10}$, his distant vision became $\frac{15}{12}$, even above normal, and he read brilliant type readily with a $+\frac{1}{8}$ lens. His spectacles were $+\frac{1}{12}$, which accounted for his discomfort. At first sight the injured right eye presented every appearance of a successful cataract extirpation.

The iridectomy was large, well shaped, centrally located, and clean to the very ciliary border. The well-formed artificial pupil was black, with traces of capsular deposit as is seen after successful cataract operations. He could detect large objects and count fingers readily at four feet, the distance at which I was sitting from him.

In putting a $+\frac{1}{8}$ lens before his injured eye I was surprised to find V. $\frac{15}{15}$, and with a $+\frac{1}{2}$ of $\frac{1}{8}$ lens he could make out words in brilliant type.

He complained of something moving about in his eye, which seemed to wave before his sight. By oblique illumination I could see a whitsh body in the vitreous. With ophthalmoscopic illumination the examination showed a healthy fundus and clear vitreous, so as to give a perfect retinal picture.

Hanging from the roof of the vitreous chamber at some little distance behind the thin, partial capsular film, was a flap of membrane apparently one line wide and two lines in length. It was rectangular in shape and of a yellowish-white color. Upon its anterior surface could be clearly traced a vessel of some size, which, starting at the base, ran down through the whole length of the membrane, to the free extremity of the flap.

This piece of living tissue moving to and fro in the vitreous with the movements of the eyeball, and adhering to the upper anterior edge of the choroid, could be nothing else than the missing piece of iris.

The splinter of wood in entering the eye through the upper scleral border of the cornea, had torn the iris in two places from the pupillary border to the ciliary region, in this way punching a piece out of it. This detached bit of iris had been pushed back into the vitreous chamber, carrying with it a portion of its ciliary body. It remained adherent to the ciliary body at one point by a broad base.

The lens must have been injured also at the time of the accident, because through its torn capsule the aqueous humor had been brought in contact with it, resulting in its complete disappearance by absorption.

As there was a broad base to the flap, with ample nourishing bloodvessels, the piece of displaced iris had continued to live. Under ophthalmoscopic examination it showed boldly as a whitish-yellow membrane against the healthy red reflex of the fundus. Curiously, the passage made by the splinter into the vitreous chamber had not been invaded by inflammatory deposits. The hyaline structures had taken on no pathological changes, and, therefore, excellent vision had been retained in this curiously injured eye.

DR. J. EDWIN MICHAEL then related

AN UNUSUAL CASE OF GUNSHOT WOUND.

On Christmas night last, about nine o'clock, he was called to see a large, stout, well-built German man, a

saloon-keeper by occupation, who had received a pistol wound, while engaged in quelling a disturbance in his establishment.

The ball came from a pistol of the "Bull Dog" pattern, 38 calibre, and was shot from a distance of twelve to fourteen feet in front and a little to the right of the patient.

Upon examining the patient it was seen that the point of entrance of the ball was at a spot almost if not directly over the apex of the heart.

Later measurements showed the exact location of the perforation to be three inches to the left of the median line, an inch and a half to the right of left nipple line, and three-quarters of an inch below a line drawn transversely through the nipple. There was, of course, no probing. The patient was in a condition of extreme shock; stimulants were given, and after a short time consciousness returned. Upon examination by physical signs, a wound of the lower lobe of the left lung and hemorrhage into the pleural cavity of that side was diagnosticated. It was also probable that there was a coexisting pneumothorax of small degree. About eleven o'clock, two hours after the accident, the patient began to complain of much general pain. He became quiet after a hypodermatic of morphia and atropia, and continued comparatively easy for a time, when pain again began to be felt, which was arrested by a repetition of the hypodermatic injection.

At the end of twenty hours from the time of the accident, the patient died.

The autopsy showed that the bullet had passed through the fifth rib, near its junction with the cartilage, through the pericardium, through the left ventricle, making an opening at its point of entrance and another at its point of exit, which two openings were connected by a furrow in the ventricular wall, ploughed up by the ball in its passage through the heart. After leaving the heart, it had penetrated and passed through the lower lobe of the left lung, fractured the eleventh rib, and embedded itself in the soft tissues of the back, about two inches to the left of the spinal column.

He thought it remarkable that such a ball should have passed through the heart without causing instant death. He had seen one other case in which the heart had been perforated by a 22 calibre ball. In this case death was practically instantaneous.

In the case under consideration the wound was obliquely through the heart in such a way as to make valve-like openings in the ventricular walls, similar to the openings in the bladder through which the ureters pass. This form of perforation, he thought, prevented what would have been rapid hemorrhage into the pericardium, had the openings been made in a line perpendicular to the plane at the point of entrance of the ball.

During life there was considerable hemorrhage, but he did not think that it came directly from the heart, but was forced from the pleural cavity by the movements of the lung in respiration. The dressing consisted of cloths saturated in bichloride solution.

DR. J. J. CHISOLM said that, of course, we had all heard of the rare case of perforation of the heart and retention of the bullet within its cavity until found there after death. He had seen cases of injury to the heart by bullets, but none of them had lived so long as twenty hours.

DR. A. B. ARNOLD next read a most exhaustive paper on

CIRCUMCISION,

in which he treated the subject from a moral as well as from a historical and surgical standpoint.

He protested against the practice, not only because he considers it detrimental in robbing the glans of its natural protective covering, and thus exposing to irritation the very sensitive Pacinian bodies about the corona, but because the result of this exposure and consequent irritation, he thought, led to habits of masturbation.

In answer to the question, "Had any hereditary shortening of the prepuce been observed in Hebrew children as a class?" he said he had noticed nothing different in the formation of the foreskin in them from that of other children.

DR. J. EDWIN MICHAEL thanked Dr. Arnold for his paper. He had some time since read an article on the subject from a surgical standpoint. In his experience he has frequently been called upon to remove the prepuce because of its inconvenience. As to affecting the sensitiveness and irritability of the glans, he did not see that the operation made any difference whatever, for how very often do we see men with naturally exposed glans from an abnormally short prepuce. They present no greater degree of sensitiveness. He thought with some eminent authorities, that the sensitiveness of the glans is increased by circumcision of a long prepuce and the irritability decreased.

NEWS ITEMS.

HYGIENE IN JAPAN.—A Bureau of Public Health, on the plan of those existing in Germany, has just been instituted in Tokio. The Director is Prof. Ogata, who has studied for several years in Munich, Leipzig, and Berlin, especially in the laboratories of Pettenkofer and Koch.

AMERICAN AND BRITISH LAUDANUM.—The *Pharmacist* remarks that the tincture of opium of the new British Pharmacopœia is directed to contain about 0.75 per cent. of morphine, while the United States preparation contains about 1.4 per cent., if opium of mean standard strength is employed; and that, therefore, the difference in strength between the two laudanums is almost 100 per cent.—*N. Y. Medical Journal*, January 2, 1886.

NEW YORK PHYSICIANS SEEKING PROTECTION.—Drs. E. G. Janeway, A. Jacobi, Austin Flint, and twenty other physicians, have sent to the Board of Health a written protest against the liability to suits for damages which physicians are made to assume when in good faith they report supposed cases of contagious diseases. A recent suit against a physician, who reported that a woman had smallpox when she was suffering from another disease, was the cause of the protest. It was said that physicians might neglect to report dangerous cases of typhus fever or smallpox if something was not done to change the law. The Board was urged to prepare a bill for the Legislature to exempt physicians from the legal consequences of too hasty reports in cases of suspected contagion. No action was taken on the communication.

ALCOHOL AND DISEASE.—Dr. Dawson Burns, the Honorary Secretary of the London Temperance Hospital, referring to the statement made to the Local Board of Winsford by the District Medical Officer that, in his opinion, three men who lately died from what seemed an attack of cholera, owed their deaths to the refusal to take alcoholic liquor, calls attention to the fact that the alcoholic treatment of cholera has confessedly proved a failure, and that recoveries have been far more numerous where it has been dispensed with. It is an admitted fact that the ordinary use of alcoholic drink, instead of being a preventive of that disease, rather predisposes to it. The superstitious value attached to alcohol in the treatment of disease is fast disappearing from enlightened medical circles; that the use of alcoholics in the great London hospitals is largely diminishing, with good results; and that the experience of the London Temperance Hospital, where alcohol has been given in only three out of three thousand in-patient cases, with an average annual mortality of five per cent. during twelve years, is exposing the fallacy under which the medical officer of Winsford appears still to suffer. —*British Med. Journ.*, Jan. 2, 1886.

CREMATION IN PARIS.—Much further interest, we learn from the *British Medical Journal* for Jan. 2, 1886, continues to be excited in Paris by the recent decision of the Municipality to erect a crematorium at Pére la Chaise, for which plans have been accepted. It is settled that the expense of cremation is not to exceed fifteen francs, and the time required for combustion will be two hours. It is in contemplation to erect a suitable building for the reception of urns or other funereal vessels containing the ashes of dead relatives, and the artistic world is already discussing the æsthetic side of cremation, while M. Koecklin Schwartz points out that persons of all denominations may unite in erecting a vast mausoleum, in which the ashes of thousands could be deposited in beautiful vessels without injury to the living. This is, however, looking rather far into the future.

THE TREATMENT OF EPILEPSY.—The prize of 8000 francs offered by the Belgian Academy of Medicine for an elucidation by clinical facts of the pathogenesis and treatment of diseases of the nervous centres, and principally of epilepsy, has not been awarded; but a sum of 500 francs is offered by way of encouragement to the author of the essay bearing the motto, "Traditionem pondero, doctrinam respicio, sequor veritatem," on condition that he allow his essay to be printed in an abridged form with the criticisms of the Commission.—*Lancet*, January 2, 1886.

ABSENCE OF HYDROPHOBIA IN BERLIN.—M. Paul Gibier, who has continued his studies of hydrophobia in the laboratory of Koch, writes to the French Academy of Medicine that he has been absolutely unable to obtain a rabid dog in Berlin, and that the physicians in the Imperial Veterinary School in that city state that they have not seen a rabid dog for more than three years.

M. Gibier attributes this condition of affairs to the fact that *in Berlin all dogs are muzzled.*

A PHYSIOLOGIST'S WILL.—On the first page of the will of the late Prof. Robin is the following request to the heirs:

"If I die in Paris, let my autopsy be made at the earliest possible moment after my death, by one of the prosectors or assistants of the École de Médecine, to be designated by the Dean. My brain and eyes should be removed and studied comparatively. The left eye was destroyed by a blow in 1835, and the optic nerves and convolutions should be studied with this fact in view."

PRESIDENCY OF THE FRENCH ACADEMY OF MEDICINE.—M. Sappey, Professor of Anatomy in the Paris Faculty, has just been elected President of the Académie de Médecine.

DR. AUGUSTIN M. FERNANDEZ, late editor of *El Repertorio Médico* of New York, has removed to New Orleans. He has recently been elected Corresponding Member of the Academy of Medicine of Madrid, Spain, and of *El Circulo Médico Argentino* of Buenos Ayres, Argentine Republic.

JUVENILE INTEMPERANCE IN LONDON.—The Bishop of London, in addressing the metropolitan members of the United Kingdom Band of Hope, congratulated himself and his fellow-teetotallers on the number of children growing up in London on temperance principles. This is a good fact, upon which society is, indeed, to be congratulated. There are few subjects on which mediual authorities are more agreed than in thinking that children are far better without any form of alcoholic stimulant, and juvenile education of this sort is the best counteractive of that deeply inherited vice of English children, which is the result of what Dr. Valpy French calls, in his interesting book, "Nineteen Centuries of Drink in England." Recent Christmas days have given painful evidence that all London boys, or girls either, are not of the Band of Hope. It is a pitiable sight to see boys and girls of sixteen or eighteen reeling along the streets. All honor to the Bishop and his coworkers, who, we trust, will be supported by hosts and hostesses in the remaining holidays. The beverage at all children's entertainments should be non-alcoholic.—*Lancet*, Jan. 9, 1886.

A DEATH FROM CHLOROFORM has occurred at Tunstall, England. The anæsthetic had been administered twice to the deceased on previous occasions, and he had come well out of it, but the third time proved fatal.

A NEW CHAIR.—Dr. Lenoir has been appointed to the newly created chair of Cutaneous and Syphilitic Diseases, at Lille.

OFFICIAL LIST OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE-HOSPITAL SERVICE, FOR THE TWO WEEKS ENDING JANUARY 16, 1886.

SAWTELLE, H. W., *Surgeon.*—To proceed to Los Angelos and Wilmington, California, as inspector, January, 1886.

GODFREY, JOHN, *Surgeon.*—To proceed to Chattanooga, Tenn., and Rome, Ga., as inspector, January 8, 1886.